The Long-term Decline of the U.S. Job Ladder

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Abstract

We argue that structural changes to the U.S. labor market over the past 40 years have lowered real wage growth by 3.3 percentage points by reducing job-to-job mobility toward higher paying jobs. A textbook job ladder model—where workers occasionally receive outside job offers and experience job separations—predicts that the gap between the distribution of wages among hires from unemployment and the overall wage distribution reflects the intensity of competition for employed workers. Using *Current Population Survey* data from 1982 to 2022, we document that this gap has narrowed substantially since the mid-1980s, suggesting a decline in net mobility toward higher paying jobs. Estimating an extended quantitative version of the textbook model, we find that increased job-to-job mobility into lower-paying (but potentially higher-value) jobs has reduced real wage growth by 0.6 percentage points, while a decline in upward mobility toward higher-paying jobs has contributed a 2.5 percentage point decline. Of this, lower aggregate matching efficiency accounts for a 1.3 percentage point reduction, decreased search efficiency or intensity among employed workers explains 0.9 percentage points, and rising employer concentration contributes 0.6 percentage points.

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1 Introduction

Over the past four decades, the real wage of the average American worker has barely grown. A vast literature attributes this stagnation to technological change (Acemoglu and Restrepo, 2020), globalization (Autor, Dorn and Hanson, 2013), and institutional factors, including the real erosion of the federal minimum wage (Autor, Manning and Smith, 2016). Comparatively less attention has been paid to the role of structural changes in the labor market and their empirical manifestation in worker flows, despite compelling evidence that mobility toward higher-paying jobs is a critical determinant of individual wage growth (Topel and Ward, 1992). This paper quantifies the contribution of changes to the structure of the labor market to wage stagnation over the past 40 years, finding that a secular decline in net mobility toward higher-paying jobs has reduced cumulative real wage growth by 3.3 percentage points between 1982–1991 and 2012–2021.

Our starting point is a textbook partial equilibrium job ladder model. In the model, unemployed workers receive job offers at some rate, with an associated wage drawn from a *wage offer distribution*. The wage remains fixed until separation, which occurs for one of two reasons. First, the worker may receive and accept a higher-paying outside offer, again drawn from the wage offer distribution. Second, the job is destroyed due to an obsolescence shock, in which case the worker either transitions directly to another job with some probability or otherwise becomes unemployed.

A central prediction of the model is that the wage distribution first-order stochastically dominates the wage offer distribution. The happens as workers systematically transition toward higher-paying jobs—they *climb the job ladder*. Moreover, the degree of competition for employed workers, captured by the arrival rate of outside job offers, determines the extent of this dominance. When competition is more intense, wage growth accelerates, widening the gap between the wage and wage offer distributions. Thus, the magnitude of this gap provides a direct empirical measure of competition for employed workers in the labor market.

Exploiting the structural mapping implied by theory, we consistently measure the intensity of competition for employed workers from January 1982 to December 2022 based on microdata from the *Current Population Survey* (CPS). Specifically, we first estimate residual hourly wages, controlling for a rich set of demographic characteristics—including race, gender, age, education, state, and three-digit occupation—each flexibly interacted with year. We then nonparametrically estimate both the wage and wage offer distributions, identifying the latter as the distribution of wages among workers who were non-employed in the previous month.

Consistent with our theory, we find that the wage distribution first-order stochastically dominates the wage offer distribution, reflecting workers' upward mobility toward higher-paying jobs. However, the gap between the two distributions has declined substantially since the mid-1980s. Interpreted through the lens of the model, this trend suggests a marked decline in net mobility into higher-paying jobs. Our estimates indicate that, in 1985, a worker on average received one outside job offer between two separation events, whereas by 2022, this figure had fallen below 0.5.

There are several possible explanations of this pattern. For instance, a more turbulent macroeconomic environment—reflected in more frequent separations rather than reduced mobility into higher-paying jobs—may drive the trend. If so, we might also expect an increase in employment-to-non-employment (EN) mobility. However, the EN rate has actually fallen modestly over this period. Another explanation is that non-employed workers today are better at identifying good job matches, leading to improved matching upon reemployment and fewer subsequent job transitions (Mercan, 2017; Pries and Rogerson, 2022). If this were the case, we would expect a disproportionate decline in the EN rate among re-entrants from non-employment, as fewer of them subsequently learn that their match is poor. We find no evidence to support this hypothesis.[1] A third possibility, not captured by the stylized model, is wage growth within jobs. If wages grow less with tenure today, this could explain the convergence of the wage and wage offer distributions. However, the consensus in the literature is that such tenure effects are modest (Altonji and Williams, 2005), which we confirm. Moreover, they show no distinct time trend.

Several alternative explanations remain plausible. To quantify their relative importance as well as uncover the deeper structural forces at play and assess their aggregate implications, we develop and estimate an extended quantitative model that, among others, incorporates on-the-job wage dynamics, a finite number of employers, and unobserved heterogeneity in both earnings ability and non-employment incidence. Non-employed workers receive job offers from a type-dependent wage offer distribution. Once employed, wages evolve according to the continuous-time counterpart of an AR(1) process. Workers may experience brief non-employment spells before being recalled by their previous employer (Fujita and Moscarini, 2017), while permanent job separations occur at a type-dependent rate. Employed workers receive outside job offers at a rate determined by the interaction of aggregate matching efficiency, labor market tightness, the relative search intensity of employed workers, and the number of recruiting employers in the market.

We estimate eight parameters separately by decade: 1982–1991, 1992–2001, 2002–2011 and 2012–2021. A crucial source of identification comes from the joint distribution of workers over wages in their first and second *Outgoing Rotation Group* (ORG) interviews, recorded 12 months apart. We exploit these joint distributions separately for all workers, those who remained with their employer, those who experienced a period of non-employment between wage observations, and those who were non-employed just before their first ORG interview. To recover the average number of recruiting employers in a market, we use cross-state variation. Specifically, we project state-year estimates of flow hazards on the number of firms per worker from the U.S. Census Bureau's *Business Dynamics Statistics* (BDS), controlling for state and year fixed effects.

Our estimates indicate a 45 percent decline in the arrival rate of higher-paying job offers between the 1980s and the 2010s. While this decline parallels the drop in the job-finding rate of the non-employed, the former has been more pronounced due to a 22 percent decline in relative search intensity or efficiency of the employed and an eight percent fall in the number of recruiting

[1] Molloy et al. (2016) similarly conclude that re-entrants are not better matched today based on trends in entry wages.

employers. The latter has constrained workers' ability to pursue outside offers. The decline in the job-to-job mobility rate has been less severe, for two reasons. First, as the frequency of higher-paying job offers has declined, workers have become increasingly mismatched, raising their likelihood of accepting outside offers. Consequently, the voluntary transition rate into higher-paying jobs has fallen by 10 percentage points less than the arrival rate of such offers. Second, voluntary transitions to higher-paying jobs represent only a subset of overall job-to-job mobility, and we estimate a modest increase in job-to-job transitions driven by anticipation of future separations. These findings highlight the limitations of relying solely on reduced-form outcomes, such as the aggregate job-to-job mobility rate, to infer structural changes in the labor market.

Using the estimated model, we conduct a series of counterfactual exercises in order to isolate and quantify the impact of various changes to the structure of the U.S. labor market on aggregate wage growth over the past 40 years. We find that had job-to-job mobility parameters remained at their 1980s levels, cumulative real wage growth between 1982–1991 and 2012–2021 would have been 3.3 percentage points higher. As a point of reference, Karabarbounis and Neiman (2014) document that the aggregate labor share has fallen by about five percentage points between 1980 and 2012. Assuming that the decline in net mobility toward higher paying jobs had no impact on growth in output, a back-of-the-envelope calculation would suggests that it accounts for more than half of the fall in the labor share over this period.

Further dissecting the impact of changes in job-to-job mobility, a higher rate of job-to-job mobility not directed toward higher paying jobs has lowered real wage growth by 0.6 percentage points. To the extent that these transitions improve other valuable job attributes, the change in welfare might be less severe than the decline in real wage growth would suggest. More important quantitatively has been a decline in voluntary job-to-job mobility toward higher-paying jobs, which has reduced real wage growth by 2.5 percentage points. This was not the result of less labor demand, which has rebounded after a decline up to the 2000s.

Instead, it is due to three forces. First, the efficiency with which the labor market matches searching workers with open jobs has declined, lowering the arrival rate of higher-paying offers to workers. This decline has reduced real wage growth by 1.3 percentage points. Second, we estimate that employed workers search less or less efficiently for outside job offers today, which has lowered real wage growth by 0.9 percentage points. One possible factor behind this decline is the increasing use of non-compete agreements, which might discourage on-the-job search. Third, rising employer concentration has restricted workers' ability to shop for jobs, depressing real wage growth by 0.6 percentage points. Our findings underscore the central role of changes in labor market structure in shaping wage dynamics over the past 40 years in the U.S.

Literature. This paper relates especially to three strands of literature. First, a literature estimates models of wage and mobility dynamics, building on the early work of Eckstein and Wolpin (1990) and subsequent prominent contributions by Bontemps, Robin and den Berg (2000), Christensen et al. (2005), Altonji, Smith and Vidangos (2013) and Bagger et al. (2014). Most closely related to this paper is Jolivet, Postel-Vinay and Robin (2006), who—based on estimation of a prototypical search model on data from several advanced countries—conclude that "cross-sectional data on individual wages contain the basic information needed to obtain a reliable measure of the 'magnitude of labor market frictions', as measured by a parameter of the canonical job search model." We reach a similar conclusion in a significantly richer model of earnings and mobility dynamics.

Second, a recent literature analyzes the impact of worker mobility on inequality in an environment with labor market frictions. Autor, Dube and McGrew (2023) argue that increased competition for labor in the post-Covid period has particularly raised earnings for low income workers by providing them greater opportunities to move up the job ladder, a finding that we confirm. Although they do not explicitly emphasize the role of job-to-job mobility, Alves and Violante (2025) show that by stimulating the economy, monetary policy can raise earnings at the bottom of the distribution. Relative to these papers, we take a long run approach, estimating that a decline in job-to-job mobility toward higher paying jobs has depressed real wage growth since the 1980s.

Third, a rapidly expanding literature studies the effects of labor market power on wages and employment (Azar et al., 2020; Prager and Schmitt, 2021; Azar, Marinescu and Steinbaum, 2022; Berger, Herkenhoff and Mongey, 2022; Benmelech, Bergman and Kim, 2022; Handwerker and Dey, 2022; Rinz, 2022; Yeh, Macaluso and Hershbein, 2022; Autor, Dube and McGrew, 2023; Caldwell and Danieli, 2024; Petrova et al., 2024). Most closely related, Bagga (2023) finds a positive correlation between job-to-job mobility and the firm-to-worker ratio in the cross-section of U.S. local labor markets. We draw a similar conclusion using long-run changes in mobility and concentration within states over time. Berger et al. (2023) examine the relationship between market concentration and worker flows in Norway, finding a negative correlation between concentration and job mobility. Our study complements their findings by providing evidence from the U.S., where institutional labor market differences may shape mobility trends in distinct ways, and by isolating voluntary job-to-job mobility that relocates workers toward higher paying employers.

This paper is structured as follows. Section 2 outlines a textbook partial equilibrium job ladder. Section 3 introduces the data and estimates the textbook model. Section 4 develops and estimates a richer quantitative model. Section 5 concludes.

2 A textbook job ladder model

We start by outlining a textbook job ladder model of worker dynamics.

2.1 Environment

Time is continuous and infinite, and the economy is in its long-run steady-state. A unit mass of ex-ante identical workers move across jobs as well as in and out of employment.

A mass u of non-employed workers receive job offers at rate λ. A job offer is a draw of a (log) wage w from a continuous *wage offer distribution* over support $w \in (-\infty, \infty)$ with cumulative distribution function (CDF) $F(w)$ and probability density function (PDF) $f(w)$. We assume that workers prefer work over non-employment at any wage in the support of wages.[2]

Employed workers earn a wage w at each instant over the period for which they are employed. They receive outside offers at rate $\phi\lambda$, where $\phi \geq 0$ is the *relative search intensity* of employed workers. Offers are again drawn from the distribution $F(w)$. An employed worker accepts any offer paying a higher wage, and declines any other offer.

At rate δ, employed workers experience a separation shock. We assume that workers anticipate a pending layoff ξ periods in advance. During this period, they search for alternative employment at the same intensity as non-employed workers. Given that their existing job will soon terminate, we impose that they accept an outside offer if they receive one. Although we label this a notice period, we stress that it need not involve a formal notice—it simply reflects our view that many soon-to-be laid-off workers may expect this in advance.

These assumptions serve to generate job-to-job mobility with wage cuts, which is ubiquitous in the data. An alternative interpretation is that workers sometimes accept lower-paying jobs since they offer better non-wage amenities. With some abuse of terminology, we refer to such job-to-job mobility as "involuntary."

Workers hence make an involuntary job-to-job transition at rate $\delta e^{-\xi\lambda}$, while they transition to non-employment at rate $\delta(1 - e^{-\xi\lambda})$. We assume that no alternative labor market events take place during the notice period, which given our low estimated hazard rates is approximately true.

2.2 Competition for employed workers

In steady-state, the number of non-employed workers satisfies the flow-balance equation:

$$0 \;=\; -\lambda u \;+\; \delta e^{-\xi\lambda}(1-u). \tag{1}$$

A share λu of non-employed workers find a job, while a share $\delta e^{-\xi\lambda}(1-u)$ of employed workers experience a separation shock and do not find a new job during the notice period. The inflow of employed workers is relative to the stock of existing non-employed workers.

The CDF of wages $G(w)$ is characterized by the Kolmogorov Forward Equation (KFE):

$$0 \;=\; -\underbrace{\delta G(w)}_{\text{separation shock}} - \underbrace{\phi\lambda\big(1-F(w)\big)G(w)}_{\text{better outside offer}} + \underbrace{\lambda F(w)\frac{u}{1-u}}_{\text{hires from non-employment}} + \underbrace{\delta\left(1-e^{-\xi\lambda}\right)F(w)}_{\text{immediate new offer}}. \tag{2}$$

[2] Absent worker heterogeneity, it is natural that no firm would offer a wage below the common reservation threshold. Our analysis would carry over to the case of a binding reservation wage, since it infers net mobility toward higher paying jobs by comparing where workers start after a spell of non-employment—regardless of whether this is a truncated offer distribution or not—and where they end up in the long-run.

At rate δ, workers experience a separation shock, while at rate $\phi\lambda(1 - F(w))$, they receive a better outside offer. In either case, they leave their current wage. At rate $\lambda F(w)$, non-employed workers receive a job offer that pays at most wage w. The inflow of non-employed workers is relative to the stock of employed workers. Finally, at rate $\delta(1 - e^{-\xi\lambda})$, workers are hit by a separation shock but immediately transition to a new job drawn from $F(w)$.

We define the parameter κ as the average number of opportunities a worker has to move toward higher-paying jobs between separation shocks that set them back:

$$\kappa \equiv \frac{\phi\lambda}{\delta}.$$

It summarizes the extent to which competitive forces in the labor market exert upward pressure on wages. Combining (2) and (1) and rearranging, we obtain:

$$\kappa = \frac{F(w) - G(w)}{G(w)(1 - F(w))}. \tag{3}$$

If the wage and wage offer distributions coincide, $\kappa = 0$. However, as we will see, the wage distribution first-order stochastically dominates the wage offer distribution, so that $\kappa > 0$. The larger the gap between the two distributions, the greater is competition for employed workers.

3 Data

We use the framework above together with publicly available data from the *Current Population Survey* (CPS) to infer long-run trends in competition for employed workers.

3.1 Data sources

We use publicly available data from the *Current Population Survey* (CPS) from October 1981 to March 2023, conducted by the Census Bureau for the Bureau of Labor Statistics (BLS) and made available by the Integrated Public Use Microdata Series (IPUMS) (Flood et al., 2024). Every month, the CPS surveys roughly 60,000 households using a rotating panel design. Specifically, a household responds to the basic monthly survey for four months, rotates out of the survey for eight months, and finally returns for another four months.

For a reference week in each month, the CPS records the employment status of each household member aged 15 and older, as well as job search activities during the four weeks leading up to the reference week for those who are not employed. In addition, basic demographic characteristics of the household member are collected. We refer to these data as the *Basic Monthly Survey* (BMS), and each month in these data as BMS 1–4 and BMS 13–16.

In BMS 4 and 16, i.e., before a respondent either temporarily or permanently leaves the CPS

Figure 1: Structure of the CPS rotating panel since 1979.

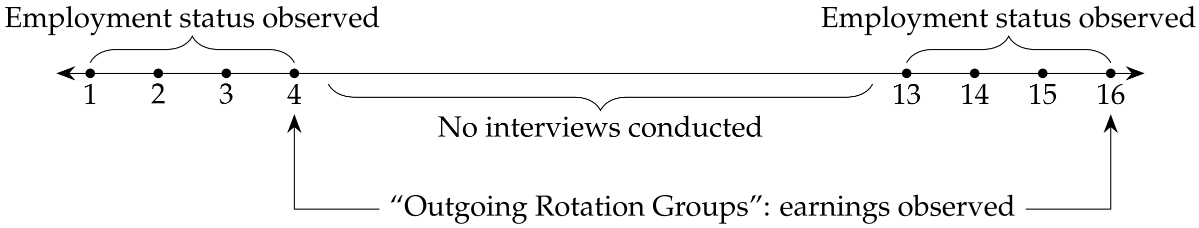


Figure 1 displays a stylized diagram of the CPS panel rotation with four months of interviews followed by an eight-month break and then another four months of interviews. The last of each of the four-month interview blocks are the so-called ORG in which earnings information is recorded.

sample, households are also asked about earnings and hours worked in the previous week. These are the so-called *Outgoing Rotation Groups* (ORG), which we refer to as ORG 4 and 16, respectively. Only wage-employed workers are asked these questions.[3]

We also use some information from the *March Supplement* of the CPS, also called the Annual Socio-Economic Supplement (ASEC). The March Supplement is fielded to any respondent who is in the sample in March. Due to how the CPS is structured, a respondent will tend to have either no March Supplement response or two. The March supplement asks a series of questions about labor market outcomes during the previous calendar year, including total wage and salary earnings and the number of distinct employers over the past year.

3.2 Variable construction and sample selection

We link individuals in the BMS 1–4, BMS 13–16, ORG 4 and 16 and the March Supplement based on household identifiers, person identifiers, age, sex, and race. Changes to individual identifiers prevent linking individuals during the June–July 1985, September–October 1985, and May–October 1995 periods. Since allocation flags generally become available in January 1982, and the Census changed how wages are recorded[4] in April 2023, we focus on the period going from January 1982, to March 2023.

Demographics. Age has been inconsistently topcoded over time, so we consistently topcode it to 75 years. When a respondent fails to provide an answer to one particular question, the CPS imputes a value for that question. We recode such allocated ages to missing and standardize age within an individual to the lowest recorded age across the 16 months an individual is potentially

[3] There are instances, however, of recorded earnings for self-employed individuals. We recode such wages to missing to be consistent.

[4] In January 2023, the Census Bureau changed how began rounding weekly earnings in an effort to improve privacy. These changes were phased in to apply only to new cohorts introduced since January 2023, and hence began affecting collected wages when the January cohort reached their fourth month in the sample, i.e. in April 2023. To avoid the break, we end our analysis prior to this date.

in the sample. We focus our analysis on those aged 20–59 years. We drop any respondent without a valid age at any point in the sample, corresponding to 3.4 percent of observations.

We aggregate race to white or non-white. Allocated responses are recoded as missing, and race is standardized within an individual (non-white if that was ever reported). Any individual without valid race at any point in the panel is dropped (this concerns 2.7 percent of observations).

We recode allocated gender to missing, and standardize it within an individual (male if they ever reported being male). Any individual without valid gender at any point in the panel is dropped from our analysis (this concerns a very small fraction of observations).

Education is aggregated to less than high school, a high school diploma, some college, a bachelor's degree, and more than a bachelor's degree. Allocated information is recoded as missing, and education is standardized to a respondent's highest reported level. We drop any individual without valid education at any point in the panel (this concerns 0.6 percent of observations). In some of our analysis below, we further aggregate education into non-college and college.

While the CPS is designed to be representative of the U.S. population, non-random attrition necessitates the use of survey weights. All our results are weighted by a respondent's average survey weight during her time in the CPS. We drop all individuals that have a zero average survey response weight, which comprises about 0.5 percent of the sample.

Occupation is recoded to the three-digit level according to the 2010 classification,[5] with allocated responses recoded to missing. Since we later control for occupation, we drop any wage employment observation with missing occupation (roughly 1.1 percent of observations).

Employment status. We classify a respondent's employment status in each month as missing, non-employed or employed. Allocated status is recoded to missing. Since the distinction between unemployment and being out of the labor force is fuzzy (Clark and Summers, 1979), we henceforth refer to all workers as either employed or non-employed. Since weekly earnings are only reported for wage and salary employees, we recode self-employed observations as missing. The employed category includes both private and public wage employees. A hire from non-employment occurs whenever someone who is wage-employed in month t but non-employed in month $t-1$.

Job stayers. To separately observe wage dynamics among those who remain with the same employer, we use information from the March Supplement on how many employers the respondent had during the previous calendar year as well as how many weeks they worked. Any allocated response is recoded as missing. We define a respondent as a *job stayer* if, in their second March Supplement response, they reported having only one employer and working 52 weeks or more during the previous calendar year.[6] The structure of the CPS complicates the measurement of

[5] We use `occ2010`, a variable which recodes reported occupations consistently to the Census 2010 occupational code.
[6] We have alternatively considered a threshold of 50 weeks, with similar results.

wage dynamics of stayers, since we cannot determine based on the March responses whether a worker remained with the same employer between ORG 4 and ORG 16. Instead, we define a worker to be a stayer between ORG 4 and ORG 16 if they were recorded as a stayer in their second March Supplement.

To give a concrete example of the complications this gives rise to, consider someone who entered the survey in December of year $t-1$. They took their ORG 4 and first March Supplement in March of year t and their ORG 16 as well as their second March Supplement in March of year $t+1$. If they were recorded as a stayer based on their second March Supplement response, it means that they remained with the same employer between January and December of year t. We do not know whether they remained with the same employer between January and March of year $t+1$, and hence between ORG 4 and ORG 16. Nevertheless, the fact that they stayed with the same employer for nine of the 12 months between ORG 4 and ORG 16 provides valuable information in our structural estimation, where we can replicate these features of the data.

Wages. Earnings are reported before taxes and other deductions and include overtime pay, commissions, and tips. For multiple-job holders, the data reflect earnings at their main job. Those who are paid by the hour report hourly pay, while salaried employees report usual weekly earnings. Respondents are also asked about usual weekly hours worked at their main job.[7] Earnings are topcoded at thresholds that vary throughout the sample, while usual weekly hours are topcoded at 99 hours.

We construct the hourly wage as that reported by those paid by the hour and as usual weekly earnings divided by usual weekly hours worked for salaried workers. We convert wages to 2022 USD using the CPI. We multiply top-coded wages by 1.5. To limit the impact of outliers, we winsorize real hourly wages at \$2.13, following Autor, Dube and McGrew (2023).

To identify imputed variables, the Census Bureau provides allocation flags. For earnings, however, such flags are missing for the period from January 1994 to August 1995, and they are incorrect between 1989 and 1993. For these years, we infer whether a variable is allocated by comparing its *edited* to its *unedited* counterpart in the underlying source data. We recoded allocated earnings to missing, except for January 1994 to August 1995, when we cannot identify them. We also recode allocated usual weekly hours worked to missing.

Since our theory concerns residual wage dispersion, we residualize wages on a rich set of observable characteristics. Specifically, we project log wages on race, gender, age, education, state,

[7]Starting in 1994, households with varying hours do not report usual weekly hours on the main job. We replace these with actual hours worked on the main job.

occupation and survey month fixed effects, all flexibly interacted with year,[8]

$$\ln wage_{it} \;=\; \alpha_{ry} \;+\; \alpha_{gy} \;+\; \alpha_{ay} \;+\; \alpha_{ey} \;+\; \alpha_{sy} \;+\; \alpha_{oy} \;+\; \alpha_{my} \;+\; \varepsilon_{it} \tag{4}$$

In our benchmark specification, we control for three-digit occupation-year fixed effects. However, since it is not clear whether cross-occupation wage dispersion should be interpreted as the result of the frictions highlighted by the theory, we also consider specifications with one-digit occupation-year fixed effects or no occupation-year fixed effects.

Let $\tilde{w}_{it}$ denote the residuals from (4). To limit the influence of outliers, whose outcomes likely do not fit well with our theory, we entirely drop individuals if their residual wage in either ORG 4 or ORG 16 is below or above the 0.5th percentile of residual wages.

Finally, we deflate residual wages in each year by the average residual wage of hires from non-employment in that year

$$w_{it} \;=\; \tilde{w}_{it} \;-\; \overline{\tilde{w}}_y^h, \qquad \text{where} \qquad \overline{\tilde{w}}_y^h \;=\; \sum_{t\in y}\sum_{i\in\mathcal{H}_t} \tilde{w}_{it}$$

where $\mathcal{H}_t$ is the set of all individuals who are employed in their ORG month but non-employed in the preceding month. Therefore w_{it} are residualized wages *relative* to average residualized wages of out of non-employment.

The wage and wage offer distributions. To construct our summary measure of competition for employed workers, κ, we require estimates of the wage and wage offer distributions. To obtain these, let $\underline{w}$ and $\overline{w}$ denote the lowest and highest residual wage, respectively, and let $\underline{b}_i$ and $\overline{b}_i$ be the lower and upper bounds for N equally spaced grid points between $\underline{w}$ and $\overline{w}$

$$\underline{b}_i \;=\; \underline{w} \;+\; (i-1)\frac{\overline{w}-\underline{w}}{N}, \qquad \text{and} \qquad \overline{b}_i \;=\; \underline{w} \;+\; i\frac{\overline{w}-\underline{w}}{N} \quad i = 1,2,\ldots,N$$

Let $w_i \;=\; .5(\underline{b}_i + \overline{b}_i)$ be the midpoints and $dw \equiv \overline{b}_i - \underline{b}_i$ be the width of each bin. We estimate the wage distribution in year y, $g_{i,y}$, as the (weighted) share of employed workers earning a wage falling within each of these bins

$$g_{i,y} \;=\; \frac{1}{dw}\frac{\sum_j \mathbb{1}_{\underline{b}_i \le w_{j,t} < \overline{b}_i} * weight_{j,y}}{\sum_j weight_{j,y}} \tag{5}$$

[8] We obtain very similar results if we alternatively include fully interacted race-gender-age-education-year fixed effects, state-year, occupation-year, and date fixed effects. Including industry-year fixed effects in (4) also makes little difference to our results.

We estimate the wage offer distribution as the (weighted) share of new hires from non-employment earning a wage falling within each of these bins

$$f_{i,y} \quad = \quad \frac{1}{dw} \frac{\sum_j \mathbb{1}_{\underline{b}_i \leq w_{j,t} < \bar{b}_i} * \mathbb{1}_{hire^n_{j,t}=1} * weight_{j,y}}{\sum_j \mathbb{1}_{hire^n_{j,y}=1} * weight_{j,y}} \tag{6}$$

We construct the CDFs of the wage offer and wage distributions as

$$F_{i,y} \quad = \quad \sum_{j=1}^{i} f_{i,y} dw \tag{7}$$

$$G_{i,y} \quad = \quad \sum_{j=1}^{i} g_{i,y} dw \tag{8}$$

We estimate our summary measure of competition for employed workers, κ_y, as the employment weighted average

$$\kappa_y \quad = \quad \sum_{i=1}^{N} \frac{F_{i,y} - G_{i,y}}{G_{i,y}(1 - F_{i,y})} g_{i,y} dw \tag{9}$$

In our benchmark, we use $N = 50$ grid points but the results are not sensitive to the exact number of grid points.[9] We obtain standard errors by bootstrapping the data 1,000 times.

3.3 Results

Figure 2 plots our estimates of the wage and the wage offer distributions by decade. In all decades, the wage distribution first-order stochastically dominates the wage offer distribution, as predicted by the theory. The extent to which it does so, however, has fallen over time.

Figure 3 plots our estimate of competition for employed workers, κ_y, based on (9) by year, treating each year as a steady-state. According to our preferred specification with three-digit occupation-year fixed effects, a worker on average made about one job-to-job transition toward a higher paying job between each separation shock in the early 1980s. Today, that figure is only half as large, indicating a marked decline in the net mobility rate toward higher paying jobs. If we control for less detailed occupations or remove occupation controls all together, we estimate a higher level of mobility but a similarly stark decline over time. The reason for the level shift across specifications is that hires from non-employment are concentrated in lower paying occupations, even after controlling for other observable demographics. Consequently, the gap between the wage and wage offer distributions is larger without occupation controls, leading us to infer a higher level of mobility toward higher paying jobs.

[9] Our results are essentially unchanged if we instead use grid points defined by percentiles of the wage distribution. We prefer the linearly spaced grids since it simplifies the numerical solution of the quantitative model.

Figure 2: Wage and wage offer distributions by decade

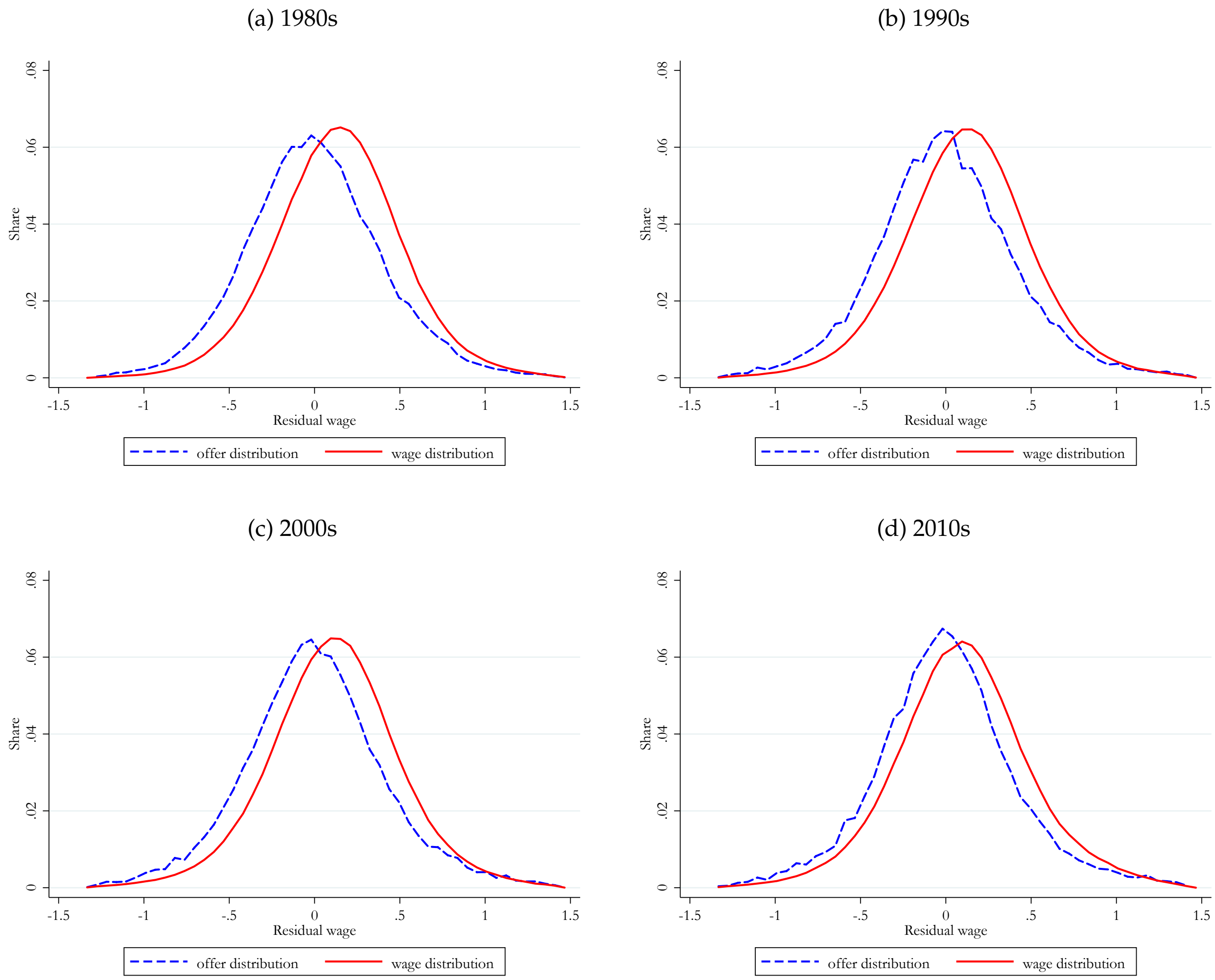


Figure 2 shows the residualized wage distribution for all workers (solid-red) and the residualized wage offer distribution of workers hired from non-employment (dashed-blue) for each of the past four decades. Observations are pooled by decade, each shown in panels (a) through (d).

Over the past 40 years, the U.S. labor force has become more female, more racially and ethnically diverse, more educated, and older. Could these demographic shifts account for the observed trends? The gap between the wage and wage offer distributions is similarly large for men and women as well as across racial groups. Consequently, controlling for changes in these dimensions barely affects our inference about competition for employed workers in Figure 3.

The gap between the wage and wage offer distributions is larger among more educated workers—consistent with their faster movement into higher-paying jobs (Deming, 2023)—and among older workers, who have had more time to climb the job ladder (Cortes, Foley and Siu, 2024). As a result, rising education levels and an aging workforce have, *ceteris paribus*, increased the gap between the wage and wage offer distributions by shifting the labor force toward groups with inherently larger gaps. Consequently, if we control for these demographic shifts, the decline in the

Figure 3: Summary measure of competition for employed workers

(a) Various occupation controls (b) Composition-adjusted

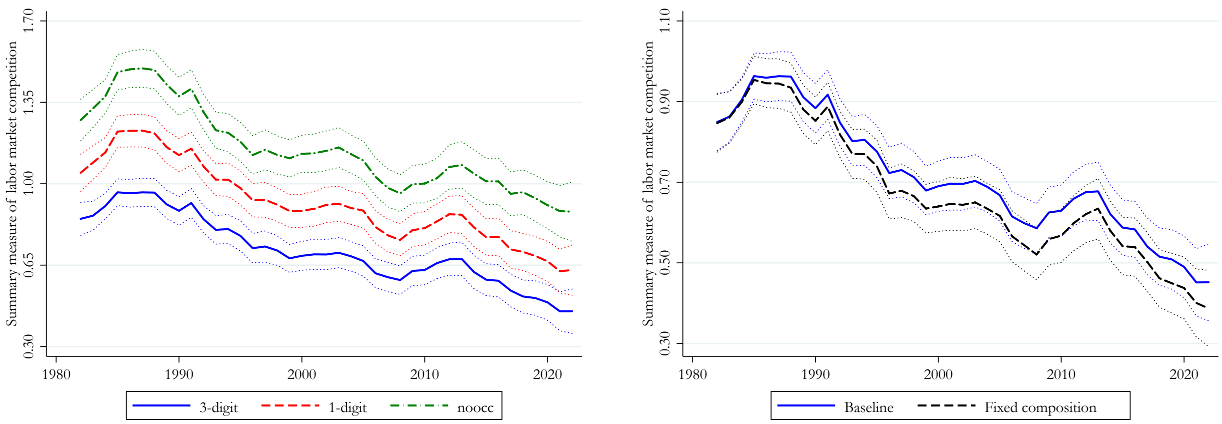


Figure 3 shows the evolution of competition for employed workers, κ, over time. Panel (a) displays measures κ for different occupation controls: without occupation controls (green-dash-dot), with 1-digit controls (red-dashed), and with 3-digit controls (blue-solid). Panel (b) shows the 3-digit occupation control measure of competition for employed workers (blue-solid) and a counterfactual measure holding the education-age composition of the U.S. workforce constant at its 1980s level (black-dotted). Dotted lines are 95% confidence intervals obtained via bootstrap (1,000 replications).

gap appears even larger, leading us to estimate a correspondingly larger decline in competition for employed workers. Panel (b) of Figure 3 illustrates this effect by holding the education-age composition of the U.S. labor force constant at its early 1980s level.

Potential explanations. While we interpret the convergence of the wage distribution toward the wage offer distribution as evidence of reduced competition for employed workers limiting their mobility toward higher-paying jobs, there are several alternative explanations. Although our structural model in the next section explicitly incorporates and quantifies many of these alternatives, we find it useful to first provide reduced-form evidence on three prominent ones.

First, our measure of competition for employed workers captures how many outside offers a worker on average receives between two separation shocks. In principle, we could infer a decline in κ not because workers receive fewer outside offers (a lower λ or ϕ), but because they are more frequently hit by separation shocks (a higher δ). Although we cannot directly measure all separation shocks in the data—in particular, we miss those that lead to a direct job-to-job transition—the separation rate into non-employment has declined over time, as shown in panel (a) of Figure 4. At face value, this is at odds with an increased separation rate driving the convergence of the wage distribution to the offer distribution (something that our richer model in Section 4 confirms).

A second possibility is that non-employed workers are better able to locate a good match today (Mercan, 2017; Pries and Rogerson, 2022). That is, suppose that workers receive a signal of the unknown quality of a prospective match, and that this signal has become more precise over time. In this case, we would expect less subsequent mobility and a convergence of the wage and wage offer distributions. We would also expect a decline in the separation rate to non-employment among newly hired workers, since fewer of them later learn that they are a poor fit with their job.

The evidence in panel (a) of Figure 4 highlights that recent hires from non-employment are more likely to experience a subsequent separation than the average worker, consistent with some realizing that they are poorly matched and terminating their match. Furthermore, the separation rate of hires from non-employment declined over time. Yet as we mentioned above, the separation rate for all workers also fell over this period, possibly reflecting a more stable macroeconomic environment that has reduced separations across the board. In relative terms, the separation rate of hires and all workers fell by similar amounts. Although more work on this is needed, this evidence leaves us skeptical that hires from non-employment are better matched today.

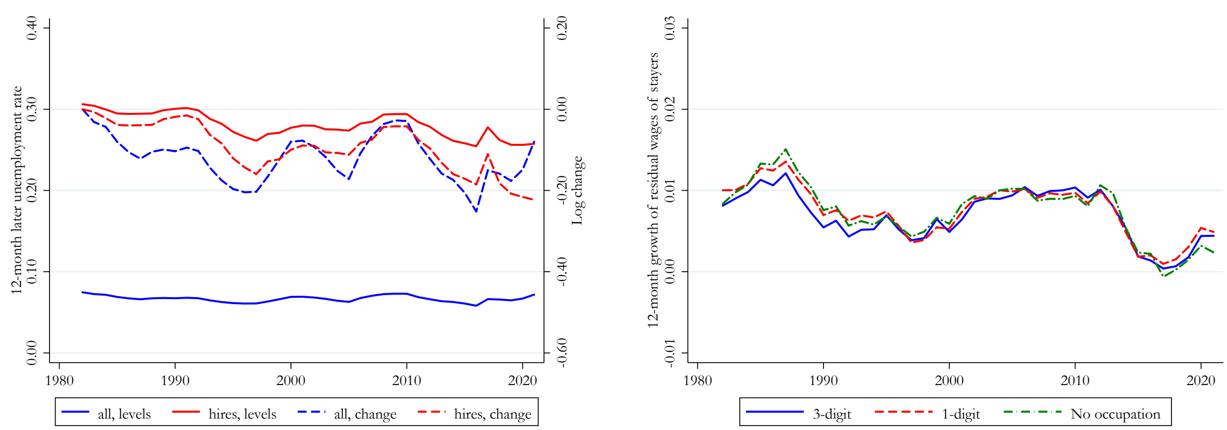
Third, the convergence of the wage and wage offer distributions could be the result of less on-the-job wage growth, which our stylized model abstracts from. We stress, however, that we are effectively comparing the wages of hires from non-employment to those of individuals *of the same age* who remained employed, thereby controlling for general wage growth with age.

Nevertheless, wages may also rise with tenure, which panel (b) finds support for. Specifically, it shows that a worker who remained with their employer throughout the previous calendar year experienced excess wage growth relative to their observationally equivalent peers. However, this tenure effect is relatively small, consistent with consensus in the literature (Altonji and Williams, 2005). Moreover, it shows no pronounced secular trend. For this reason, our structural model in the next section attributes a relatively minor role for tenure effects in driving both the gap between the wage and wage offer distributions and its change over time.

Figure 4: Potential explanations

(a) 12-month EN transition rate

(b) 12-month residual wage growth of stayers



Panel (a) of Figure 4 shows both the fraction (solid) and the change in the fraction (dashed) of employed workers who are non-employed twelve months later for all workers (blue) and for those hired from non-employment (red). Panel (b) shows the 12-month residual wage growth of workers who stayed at their jobs during the previous calendar year for the three occupation controls specifications.

Other possible explanations include changes in selection on unobservables or unemployment recall. For instance, if hires from non-employment earn less across all jobs, it would give rise to a gap between the wage and wage offer distributions. Furthermore, if such negative selection became less pronounced over time, it could explain the convergence of the two distributions. Alternatively, if layoffs followed by recall to the original employer have become more common, we might expect the wage and wage offer distributions to converge (assuming that recalled workers return at their original wage). The reason is that a larger share of those we classify as hires from non-employment are in fact returning to their original employer at a wage distributed according to the overall wage distribution. The structural theory that we turn to now incorporates and quantifies these alternative mechanisms, finding that they account for little of the secular trends.

4 A rich structural model of wage and mobility dynamics

We now expand on the prototypical model in Section 2 and estimate it using the CPS in order to quantify the contribution of various factors toward weak wage growth over the past 40 years.

4.1 Extensions

We incorporate seven extensions to the theory.

On-the-job wage dynamics. Wages on-the-job evolve according to an Ornstein-Uhlenbeck process (the continuous-time equivalent of a random walk in discrete time):

$$dw \;=\; \theta(\mu - w)dt \;+\; \sigma dW(t),$$

where θ is the autocorrelation, μ the long-run mean, σ the standard deviation of the diffusion, and $W(t)$ the standard Wiener process.[10] Then the steady-state wage distribution satisfies the KFE:

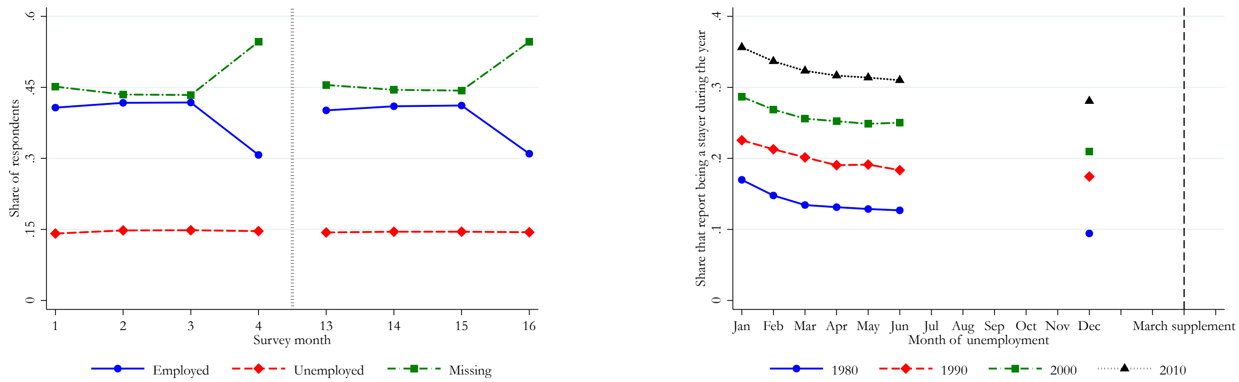
$$\begin{aligned} 0 \;=\; &-\underbrace{\delta G(w)}_{\text{separation shock}} - \underbrace{\phi\lambda\big(1-F(w)\big)G(w)}_{\text{better outside offer}} + \underbrace{\lambda F(w)\frac{u}{1-u}}_{\text{hires from } u} + \underbrace{\delta\big(1-e^{-\xi\lambda}\big)F(w)}_{\text{immediate new offer}} \\ &- \underbrace{\theta(\mu-w)g(w)}_{\text{drift}} + \underbrace{\frac{\sigma^2}{2}g'(w)}_{\text{shocks}}. \end{aligned}$$

[10]The diffusion σ is closely related to but not exactly equivalent to measurement error in wages. True measurement error would leave a worker's position on the job ladder unchanged, thereby having no impact on their labor market behavior. In contrast, the shocks we model alter a worker's position on the job ladder, thereby influencing their behavior. In practice, we have found it impossible to separately identify measurement error from wage shocks.

Figure 5: Non-response and employment measurement error

(a) Employment status by survey month

(b) Share of non-employed in month that are stayers



Panel (a) of Figure 5 shows the monthly employment status by survey month pooled over all years for employed (solid-blue), non-employed (dashed-red), and workers who do not report status (dash-dotted-green). Panel (b) shows the share of non-employed workers by month who report to be “stayers” in the second March supplement by decade (1982–1991, 1992–2001, 2002–2011, 2012–2021).

subject to the boundary conditions $\lim_{z \to -\infty} G(z) = 0$ and $\lim_{z \to \infty} G(z) = 1$, where

$$0 \;=\; -\;\lambda u \;+\; \delta e^{-\xi\lambda}(1-u).$$

Non-response. Panel (a) of Figure 5 plots monthly employment status by survey month, pooling all years of data. A non-trivial share of respondents fail to report their employment status in any given month. Furthermore, missing wage data in the ORG months increases the fraction missing in survey months 4 and 16. Surprisingly, dropout does not appear to rise with time in the survey.

To match these patterns, we assume that a respondent drops out of the survey at rate *out* and re-enters the survey at rate *in*, so that the steady-state share with missing employment status is:

$$\frac{out}{in + out}.$$

Labor market dynamics are assumed to be identical for those who have dropped out of the survey.

Recall unemployment. Non-employment displays duration dependence, which our model does not yet account for. Fujita and Moscarini (2017) argue that most of this duration dependence is accounted for by recall of non-employed workers to their previous employer. Motivated by their findings, we assume that a fraction ε of employed workers experience temporary layoffs but are recalled the following month (at their previous wage). This could alternatively be reinterpreted as measurement error in employment status (Abowd and Zellner, 1985). Allowing for changes in recall (or alternatively measurement error in employment status) is potentially important. Specifically, if a larger share of those that we classify as hires from non-employment today return to their previous employer at their previous wage—either due to recall or because they were misreported as non-employed—it would lead the observed wage and wage offer distributions to converge.

Measurement error. Panel (b) of Figure 5 illustrates that a significant share of workers who report being non-employed in survey month m of year y (based on their BMS response) later indicate in March of year $y+1$ (based on their March supplement) that they were continuously employed with a single employer throughout year y. The likelihood of such inconsistencies decreases as the reported month of non-employment approaches the date the March supplement is conducted. Specifically, it is less common for individuals who were non-employed in December of year y (according to the BMS) to later claim continuous employment than someone who was non-employed in January of year y.[11] We interpret this pattern as a recall error, which we address by assuming that a fraction ν of workers who did not remain with their empoyer throughout the year misreport their employment history, erroneously stating that they did so.

Permanent unobservable heterogeneity. Recent work points to substantial unobserved heterogeneity in labor market flows (Hall and Kudlyak, 2019; Gregory, Menzio and Wiczer, 2021). To match this evidence, we allow for two worker types, $k = \{1, 2\}$, who differ in their separation rates δ^k and wage offer distributions $f^k(w)$.

General equilibrium. Suppose that firms advertize V vacancies. Aggregate search intensity is $S = u + \phi(1-u)$. The job finding rate is given by a constant returns to scale matching function:

$$\lambda = \frac{\mathcal{M}(V,S)}{S} = \chi\left(\frac{V}{S}\right)^{\alpha}.$$

where V/S is labor market tightness and $\alpha \in (0,1)$ is the elasticity of matches with respect to vacancies. Given the difficulty of reliably estimating the latter, we impose $\alpha = 0.5$ in our analysis. We obtain data on aggregate vacancies from Barnichon (2010) merged with JOLTS data since 2001. Combined with an internal estimate of aggregate search intensity S, we recover tightness V/S.

A finite number of employers. We assume that a location is divided into M distinct labor markets, proportional to the number of workers N in that location:

$$M = \beta N.$$

[11] Due to the structure of the CPS, we are unable to link non-employment status from July to November of year y with stayer status in the March supplement of year $y+1$.

If n is the number of firms in the location, then the number of firms per market m is given by

$$m = \frac{n}{M} = \frac{n}{\beta N}.$$

Suppose that each firm advertises v vacancies and assigns to each an idiosyncratic wage according to the aggregate offer distribution. Furthermore, suppose that whenever an employed worker learns about a job opening at her current employer, she is restricted from applying to the job. These assumptions imply that the effective arrival rate of better job offers λ^e is

$$\lambda^e = \lambda\phi\frac{mv - v}{mv} = \lambda\phi\Big(1 - \beta \cdot fsize\Big), \qquad \text{where} \qquad fsize \equiv \frac{N}{n}.$$

4.2 Methodology

We estimate the model separately for CPS cohorts defined based on when a respondent first entered the CPS, allowing all parameters except β to vary across periods. For each CPS respondent i, the observed outcome vector is

$$\mathbf{x}_i = \Big\{ s_i^1 , s_i^2 , s_i^3 , w_i^4 , s_i^{13} , s_i^{14} , s_i^{15} , w_i^{16} , stayer_i \Big\},$$

where employment status s_i^m is coded as -1 (missing), 0 (non-employed), or 1 (wage-employed); wages w_i^m are binned into 50 equally spaced bins, with non-employment coded as $w_i^m = 0$ and missingx as $w_i^m = -1$; and $stayer_i$ indicates whether a respondent remained with their employer throughout the previous calendar year based on their second March supplement response, with non-response/not in the March supplement coded as -1. The estimation proceeds in three steps.

Step I. We recover a first set of parameters directly from the data. We determine the entry rate from non-response, in, by matching the fraction of observations with missing employment status in month m that report a non-missing status in month $m+1$, pooling survey months 1–3 and 13–15

$$in = \frac{\sum_i \sum_{m\in\{1,2,3,13,14,15\}} p_j \mathbb{1}_{s_{i,m}=0, s_{i,m}\neq 0}}{\sum_i \sum_{m\in\{1,2,3,13,14,15\}} p_j \mathbb{1}_{s_{i,m}=0}},$$

where p_j is a respondent's average survey response weight. The outflow rate is recovered from the steady-state flow-balance relationship:

$$out = \frac{miss * in}{1 - miss}, \qquad \text{where} \qquad miss = \sum_i \sum_{m\in\{1,2,3,4,13,14,15,16\}} p_j \mathbb{1}_{s_{i,m}=0},$$

where $miss$ represents the overall fraction of workers with missing employment status.

To estimate the job finding rate of the non-employed λ and the share of workers in recall non-employment ε, we analyze a three-month panel of workers with non-missing employment status pooling survey months 1–3 and 13–15. If u is the true aggregate non-employment rate, then the share of workers who are unemployed in the first month is:

$$\underbrace{\widehat{u}}_{\text{observed share unemployed}} = \underbrace{u}_{\text{share of truly unemployed}} + \underbrace{(1-u)\varepsilon}_{\text{share on recall}}. \tag{10}$$

The fraction observed as non-employed in the first and second month is approximately equal to:

$$\widehat{uu} = (1-\lambda)u. \tag{11}$$

This approximation abstracts from the possibility of someone making multiple transitions within the two months (including being on recall for two consecutive months). Given our low estimated flow rates, the probability of two events taking place is second-order. Finally, the share observed as non-employed for all three months is:

$$\widehat{uuu} = (1-\lambda)^2 u. \tag{12}$$

Solving equations (10)–(12) yields:

$$\lambda = 1 - \frac{\widehat{uuu}}{\widehat{uu}}, \tag{13}$$

$$\varepsilon = \frac{\widehat{uu}^2 - \widehat{u} * \widehat{uuu}}{\widehat{uu}^2 - \widehat{uuu}}, \tag{14}$$

$$u = \frac{\widehat{uu}^2}{\widehat{uuu}}. \tag{15}$$

We estimate the probability of misreporting to be a stayer ν in the following manner. First, we condition on those who are non-employed in both survey months $m-1$ and m, where $m = 1, \ldots, 6$ (i.e. January-June) as well as $m = 13$ (i.e. January of the subsequent year). By conditioning on non-employment in two consecutive months, we remove almost all workers on recall to only get those who are truly unemployed (the probability of two consecutive months on recall is vanishingly small). Among this group, we compute the share who report being reports being a stayer in the year. Call this share ν_m. We linearly interpolate using ν_6 and ν_{13} to obtain ν_m also for $m = 7, \ldots, 12$, and assign as our estimate of ν the average:

$$\nu = \frac{1}{13} \sum_{m=1}^{13} \nu_m.$$

Step II. We pick the remaining parameters via Simulated Method of Moments. Without loss of generality, we impose that the first worker type is more likely to separate $\delta^1 \geq \delta^2$. We henceforth refer to the second type as the "high" type, as we estimate that they tend to sample on average better wage offers as non-employed. While we estimate directly the two separation rates δ^1 and

δ^2, in our counterfactual exercises it is instructive to separately vary the level of the separation rate and heterogeneity in it. For that purpose, we assume that

$$\delta^1 \;=\; \bar{\delta} * \delta^s, \qquad\qquad \delta^2 \;=\; \frac{\bar{\delta}}{\delta^s}.$$

where

$$\bar{\delta} \;=\; \sqrt{\delta^1 * \delta^2}, \qquad\qquad \delta^s \;=\; \sqrt{\frac{\delta^1}{\delta^2}}.$$

If $\widehat{f}(w)$ is the observed wage offer distribution and $f(w)$ is the true offer distribution, then

$$\widehat{f}(w) \;=\; \frac{u\lambda f(w) + (1-u)\varepsilon g(w)}{u\lambda + (1-u)\varepsilon},$$

since a share λ of truly non-employed workers find a job drawn from the true offer distribution $f(w)$, while a share ε of employed workers distributed according to $g(w)$ are on recall, and are hence recorded as re-entrants in the next period. Since the true wage distribution $g(w)$ coincides with the observed wage distribution $\widehat{g}(w)$, we can recover the true offer distribution as:

$$f(w) \;=\; \frac{\widehat{f}(w)(u\lambda + (1-u)\varepsilon) - (1-u)\varepsilon g(w)}{u\lambda}. \tag{16}$$

We take the wage offer distribution (16) as a structural input into the model when solving it. In theory, this is not entirely accurate due to time-aggregation—even among those who will not be recalled to their previous employer, the true wage offer distribution does not coincide with the distribution of wages among those who were non-employed in the previous month, since those who were non-employed in the previous month have experienced other events since being hired (job-to-job mobility and on-the-job wage dynamics). In practice, however, our low estimated flow rates imply that the bias from such time aggregation is minor, as we show below.

For given type-specific separation rates δ^k, notice period ξ, and job finding rate λ, the non-employment rates for low and high types are

$$u^1 \;=\; \frac{\delta^1 e^{-\xi\lambda}}{\delta^1 e^{-\xi\lambda} + \lambda}, \qquad\qquad u^2 = \frac{\delta^2 e^{-\xi\lambda}}{\delta^2 e^{-\xi\lambda} + \lambda}.$$

We assume that the wage offer distribution of the high-type is normal (in logs) with mean equal to the mean of the true wage offer distribution μ_f (based on (16)) plus a shifter ω and standard deviation equal to the true wage offer distribution σ_f:

$$f^2(w) \;=\; \min\left\{ \frac{u^2}{u^1+u^2} x(w) \;,\; f(w) \right\}, \qquad\qquad x(w) \;\sim\; \mathcal{N}\Big(\mu_f + \omega \;,\; \sigma_f^2 \Big),$$

with the restriction that it at most has as many workers at a given wage as the true offer distribution (renormalized to integrate to one). The offer distribution of the low type is the residual

$$f^1(w) \quad = \quad f(w) \quad - \quad \frac{u^2}{u^1+u^2} f^2(w),$$

again renormalized to integrate to one.

For the purposes of estimation, we introduce the auxiliary parameters

$$\lambda^e \quad = \quad \phi\lambda \left(1 - \frac{1}{m}\right), \qquad\qquad \lambda^f = 1 - e^{-\xi\lambda}.$$

λ^e is the effective job finding rate of the employed and λ^f is the job finding rate of a worker who learns that their job will terminate in the near future.

These assumptions leave us with eight parameters, which we pick to minimize the sum of squared deviations between a set of moments $\mathcal{M}$ in the model and data:

$$\left(\widehat{\mu}\,,\,\widehat{\theta}\,,\,\widehat{\sigma}\,,\,\widehat{\lambda^e}\,,\,\widehat{\lambda^f}\,,\,\widehat{\omega}\,,\,\widehat{\delta^1}\,,\,\widehat{\delta^2}\right) \quad = \quad \underset{\{\,\mu\,,\,\theta\,,\,\sigma\,,\,\lambda^e\,,\,\lambda^f\,,\,\omega\,,\,\delta^1\,,\,\delta^2\,\}}{\arg\min} \sum_{m\in\mathcal{M}} \left(m^{\text{data}} \;-\; m^{\text{model}} \right)^2.$$

We discuss heuristically below how the set of moments we include in $\mathcal{M}$ inform each parameter.[12]

We target for the parameters governing on-the-job wage dynamics $\{\mu, \theta, \sigma\}$ the joint distribution of wages in ORG 4 and ORG 16 among workers who report that they remained with their employer throughout the previous calendar year. Since this requires the respondent to be in the March supplement, this restricts attention to those who entered the CPS between December of year $t-1$ and March of year t, and who provided a valid response in March (i.e., had not dropped out of the sample). We replicate these criteria in the model to mimic exactly the data.[13]

We target for the job finding rate of the employed λ^e the wage distribution. As highlighted by the stylized model in Section 2, a higher λ^e shifts the wage distribution further to the right of the

[12]To minimize the objective, we employ a gradient-based method starting from a set of randomly drawn points in the eight dimensional parameter space. We chose as the global minimum the local minimum that is associated with the smallest minimum distance (in practice, most starting points converge to the same minimum).

[13]Consider, for instance, someone who entered the CPS in December of year $t-1$. Based on their March supplement response in year $t+1$, we know whether they remained with the same employer between January and December of year t, but we do not know whether they stayed with the same employer between January of year $t+1$ and March of year $t+1$. We observe the respondent's wage in March of year t and March of year $t+1$, when they are in their ORG.

We hence compute in the model the share of workers that earn wage w in month three and wage $\tilde{w}$ in month 15, who stay with the same employer for twelve months, and then follow the wage dynamics of all workers for three months. To replicate those who entered the CPS in January, we compute the share of workers with wage w in month four and wage $\tilde{w}$ in month 16, who stay with the same employer for twelve months, and then follow the wage dynamics of all workers for four months. To replicate those who entered the CPS in February, we compute the share of workers that earn wage w in month five and wage $\tilde{w}$ in month 17, who stay with the same employer for twelve months, and then follow the wage dynamics of all workers for six months. Finally, to replicate those who entered the CPS in March, we compute the share of workers that earn wage w in month six and wage $\tilde{w}$ in month 18, who stay with the same employer for twelve months, and then follow the wage dynamics of all workers for six months. We add these shares together.

offer distribution. We target for the job finding rate of workers on advanced notice λ^f the share of workers that are stayers, as well as the joint distribution of workers over wages in ORG 4 and ORG 16 among all workers. Conditional on flows in and out of non-employment, a higher λ^f results in a lower share of stayers, as well as more mobility toward lower paying jobs.

We target for mean differences in wage offers between unobservable worker types, ω, the joint distributions of wages in ORG 4 and ORG 16 among workers who are non-employed at some point in BMS 13–15, as well as that among workers who were non-employed at some point in BMS 1–3. If differences in job offers across types are larger (ω is further from zero), the correlation between wages prior to a job loss and after is higher. Similarly, conditional on a given gap between the wage and wage offer distributions, a higher ω is associated with less wage growth among those who recently found a job. The reason is that more of the gap between the wage and wage offer distributions is accounted for by unobserved heterogeneity.

Finally, the aggregate non-employment rate as well as the joint distribution of wages in ORG 4 and ORG 16 among those who were non-employed at some point in BMS 13–15 informs the type-specific separation rates $\{\delta^1, \delta^2\}$. High-type workers tend to sample better offers and they are less likely to get hit by a separation shock. Consequently, high type workers are concentrated at high wages. The extent to which job losers are concentrated at the bottom of the distribution hence informs heterogeneity in δ's.

Step III. The final parameter, β, governs the number of workers per firm in a labor market. To inform this, we re-estimate a restricted version of the model by US state-year. The modest size of the CPS at the state-year level makes it difficult to obtain reliable estimates of some of the moments we use in our national level estimation such as the wage transition matrices of job losers and job finders. We hence restrict the following parameters to be the same across states, equal to their national level estimates in that year (obtained by estimating the model above by year):

$$\Big(\varepsilon_{sy} \ , \ \theta_{sy} \ , \ \sigma_{sy} \ , \ \omega_{sy} \ , \ \delta^s_{sy} \Big) \quad \equiv \quad \Big(\varepsilon_{y} \ , \ \theta_{y} \ , \ \sigma_{y} \ , \ \omega_{y} \ , \ \delta^s_{y} \Big).$$

We let the remaining parameters $\{in_{sy}, out_{sy}, \lambda_{sy}, \mu_{sy}, \lambda^e_{sy}, \lambda^f_{sy}, \overline{\delta}_{sy}\}$ vary flexibly by state-year. We externally calibrate the in and outflow from and to non-response as well as the job finding rate of the non-employed λ_{sy}, and estimate four parameters by Simulated Method of Moments:

$$\Big(\widehat{\mu}_{sy} \ , \ \widehat{\lambda^e}_{sy} \ , \ \widehat{\lambda^f}_{sy} \ , \ \widehat{\overline{\delta}}_{sy}\Big) \quad = \quad \underset{\{ \ \mu \ , \ \lambda^e \ , \ \lambda^f \ , \ \overline{\delta} \ \}}{\arg\min} \quad \sum_{m \in \mathcal{M}^s} \Big(\ m^{\text{data}}_{sy} \ - \ m^{\text{model}}_{sy} \ \Big)^2.$$

We include in the set of targets $\mathcal{M}^s$ the joint distribution of stayers over wages in ORG 4 and 16 (μ_{sy}), the wage distribution (λ^e_{sy}), the share of stayers as well as the joint distribution of all workers over wages in ORG 4 and 16 (λ^f_{sy}) and the aggregate non-employment rate ($\overline{\delta}_{sy}$).

We assume that relative search intensity of the employed is the product of a time-invariant state fixed effect, a national-level time effect and a component that may vary differentially by state over time but which is orthogonal to average firm size

$$\phi_{sy} \;=\; \phi_s * \phi_y * \tilde{\phi}_{sy}, \qquad\qquad \tilde{\phi}_{sy} \;\perp\; fsize_{sy}.$$

We then obtain the proportionality parameter β by estimating by non-linear least squares

$$\ln \widehat{\lambda^e}_{sy} \;-\; \ln \widehat{\lambda}_{sy} \;=\; \ln\left(1 - \beta * fsize_{sy}\right) \;+\; \alpha_s \;+\; \alpha_y \;+\; \varepsilon_{sy}. \tag{17}$$

Given an estimate $\widehat{\beta}$ based on (17), we recover an estimate of the number of recruiting employers at the national level in period y as

$$\widehat{m}_y \;=\; \frac{1}{\widehat{\beta} * fsize_y},$$

and an estimate of relative search intensity of the employed in period y as

$$\widehat{\phi}_y \;=\; \frac{\widehat{\lambda^e}_y}{\widehat{\lambda}_y} * \frac{\widehat{m}_y}{\widehat{m}_y - 1}.$$

Given an estimate of relative search intensity of the employed as well as a true non-employment rate u, aggregate search intensity is:

$$\widehat{S}_y \;=\; u_y \;+\; (1 - u_y)\widehat{\phi}_y.$$

Using our estimate of the job finding rate of the non-employed λ from Step I, aggregate vacancies from Barnichon (2010)/JOLTS and our pre-set value of the curvature of the matching technology $\alpha = 0.5$, we recover an estimate of matching efficiency as

$$\widehat{\chi}_y \;=\; \lambda_y \left(\frac{\widehat{S}_y}{V_y}\right)^{\alpha}.$$

4.3 Model fit

We begin by demonstrating that the model effectively captures a broad set of labor market dynamics at each point in time. Given our focus on long-run secular trends, we focus primarily on an estimation based on decade-long data: 1982–1991, 1992–2001, 2002–2011, and 2012–2021 (as defined by the year a respondent enters the CPS).

Figure 6 presents the wage and offer distributions in both the model and the data across these decades. Although we feed into the model the wage offer distribution (16), the resulting wage offer distribution in the model does not necessarily match the empirical one due to time-aggregation effects. Specifically, we measure the resulting wage offer distribution in the model as the distribution of wages among those who were non-employed in the previous month, consistent with the data. Since the continuous-time model allows for within-month events, the wage offer distribution that we feed into the model does not necessarily align with the measured distribution. However, in practice, the two distributions align closely due to the model's low estimated flow rates. The model also successfully reproduces the empirical wage distribution, with the exception of the far right tail, where it struggles to account for the relatively large share of workers earning more than 100 log points above the average residual wage offer.

Figure 6: Wage offer and wage distributions in model and data

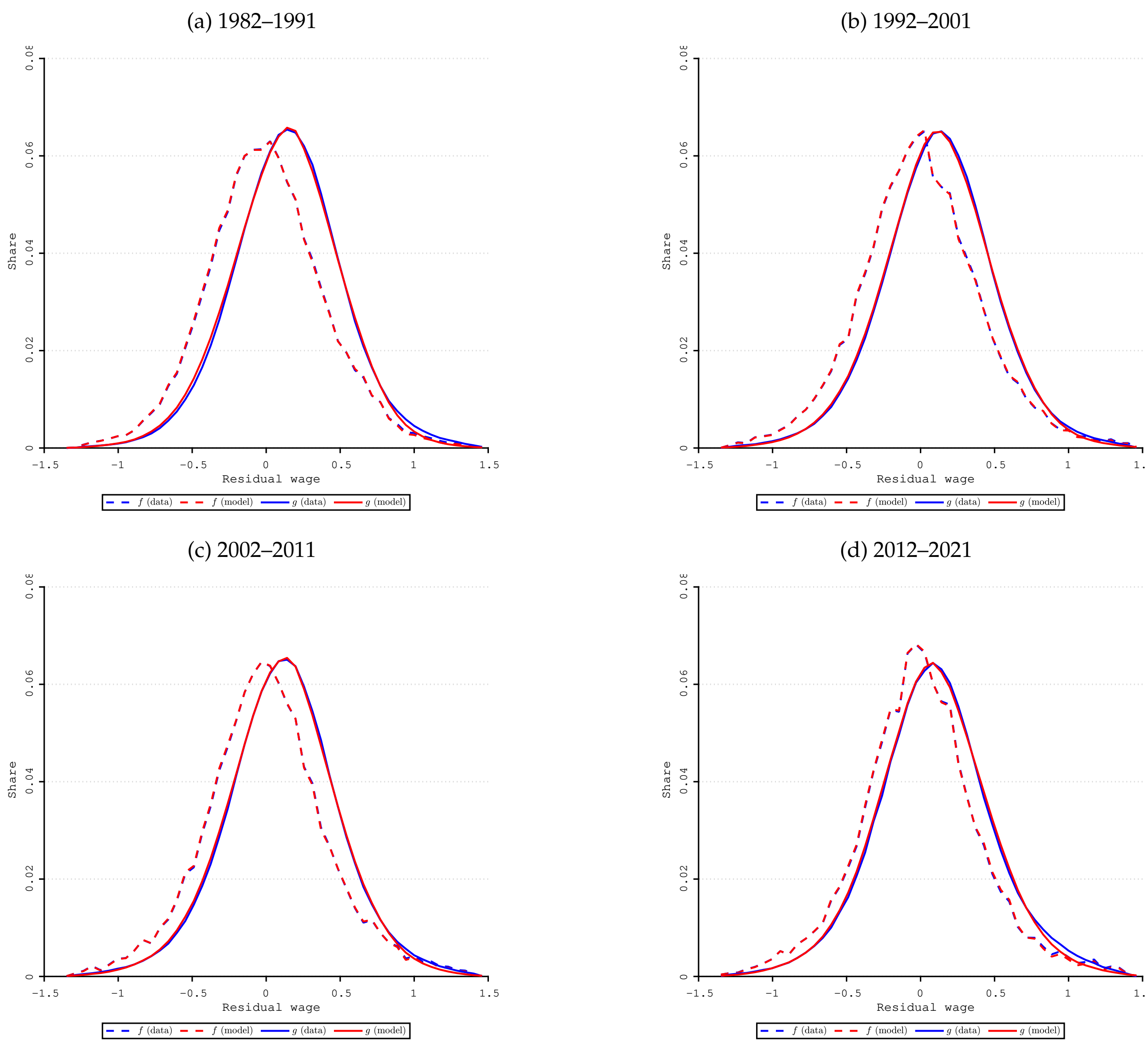


Figure 6 shows the model fit and the empirical counterpart for the wage offer distribution (data shown in dashed-blue and model shown in solid-red) and the wage distribution (data shown in dash-dotted-green and model shown in dotted-black) for the past four decades. Observations are pooled by decade, each shown in (a) panels through (d).

Panels (a)–(b) of Figure 7 depict the joint distribution of workers' wages in ORG 4 and ORG 16, conditional on non-missing wages in both surveys. For conciseness, we display results only for the most recent decade (2012–2021), though patterns are consistent across decades. The model accurately replicates the joint distribution observed in the data despite its parsimonious parameterization. Panels (c)–(d) compare the joint wage distribution for stayers relative to all workers, demonstrating that stayers are concentrated at higher wages in both model and data.

Figure 7: Joint distribution of all workers in model and data

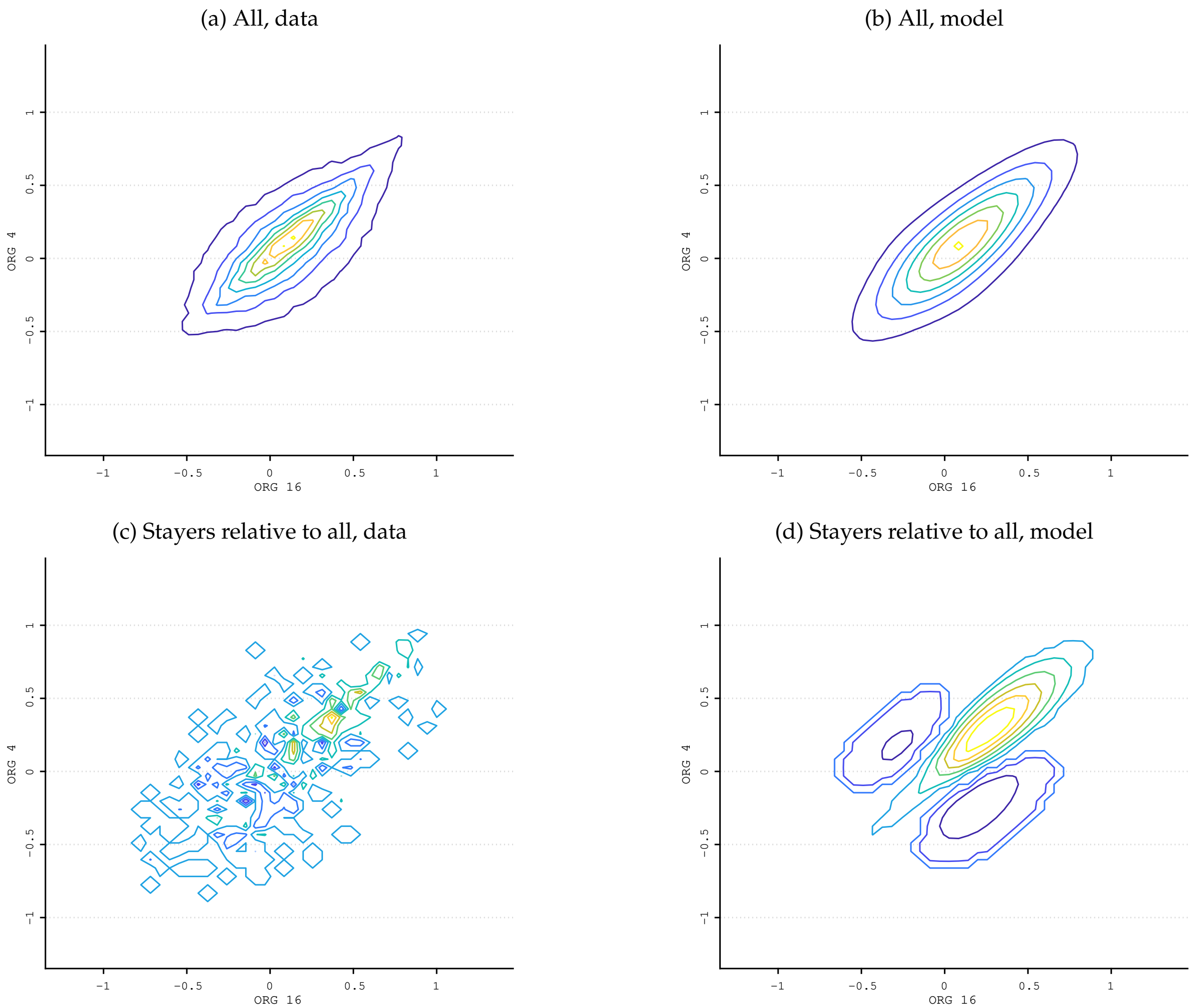


Figure 7 shows untargeted model and data moments. Panels (a) and (b) of the figure show the join-distribution of wages for all workers in ORG 4 and ORG 16 for workers with non-missing wage in the data and the model, respectively. Panels (c) and (d) of the figure show the joint distribution of wages for stayers — as indicated in the second March supplement for the previous calendar year — for the data and the model, respectively. Lighter colors indicate higher density.

Panel (a) of Figure 8 plots the average residual wage in ORG 4 of hires from non-employment in ORG 16. In both the model and data, we include workers who were non-employed in at least one of BMS 13-15. Recent hires from non-employment earned a lower wage in their previous job. Permanent unobservable heterogeneity allows the model to match this pattern well. Panel (b) illustrates growth in residual wages between ORG 4 and ORG 16 for workers that were hired from non-employment in ORG 4. In both the model and data, we include workers who were non-employed in at least one of BMS 1-3. Recent hires from non-employment experience excess wage growth relative to their identical looking peers, consistent with them recovering after a separation by relocating up the job ladder. As such opportunities have dwindled, the extent of this excess wage growth has fallen over time. The model matches well the data.

Figure 8: Selection on unobservables over time in the model and data

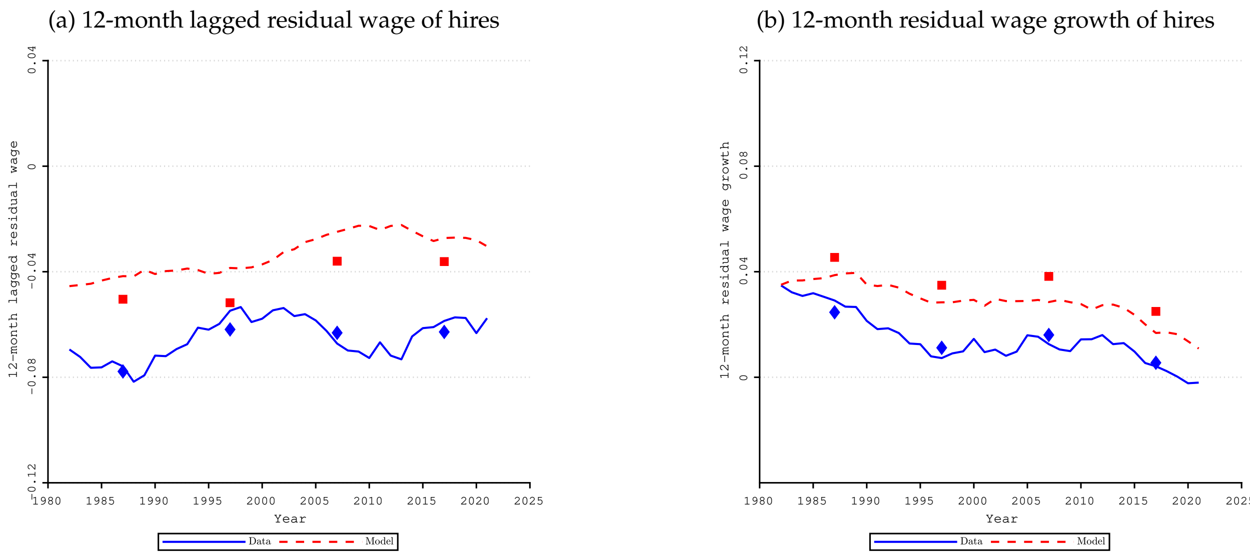


Figure 8 shows the evolution of key indicators of selection on unobservables in the model and data. Panel (a) compares residual wages in ORG 4 for workers hired from non-employment in ORG 16 between data (solid-blue) and model (dashed-red), with decade-pooled estimates (blue diamonds for data, red squares for model). Panel (b) shows residual wage growth between ORG 4 and ORG 16 for workers hired from non-employment, with the same markers for pooled estimates.

Figure 9 contrasts some additional model outcomes with the data, focusing on the 2012–2021 period. Panel (a) shows that workers earning higher wages in ORG 4 are less likely to be non-employed in ORG 16. To highlight variation across the wage distribution, we express this relative to the EU rate at the midpoint on the wage grid. The model successfully replicates the decline in the EU rate with wages, driven by sorting of high-type workers into high-paying jobs with lower intrinsic separation rates. The model slightly underestimates this gradient, which could be due to unmodeled differences in job separation rates across wage levels.

Panel (b) depicts the share of workers by wage in ORG 4 who remained with the same employer throughout the calendar year. Due to how the CPS is structured, the ORG 4 wage is not the wage at the beginning of the calendar year (the model exactly replicates how the data are constructed). This feature explains why the probability of being a stayer declines at the top of the wage distribution: some workers transition to higher-paid jobs within the calendar year prior to their ORG 4, leading them to be recorded as a mover in the calendar year and earning a high ORG 4 wage. The model slightly overstates the gradient between wages and stayer probability.

Figure 9: Additional model outcomes

(a) Relative EU mobility by wage (b) Share of stayers by wage (c) Months of employment

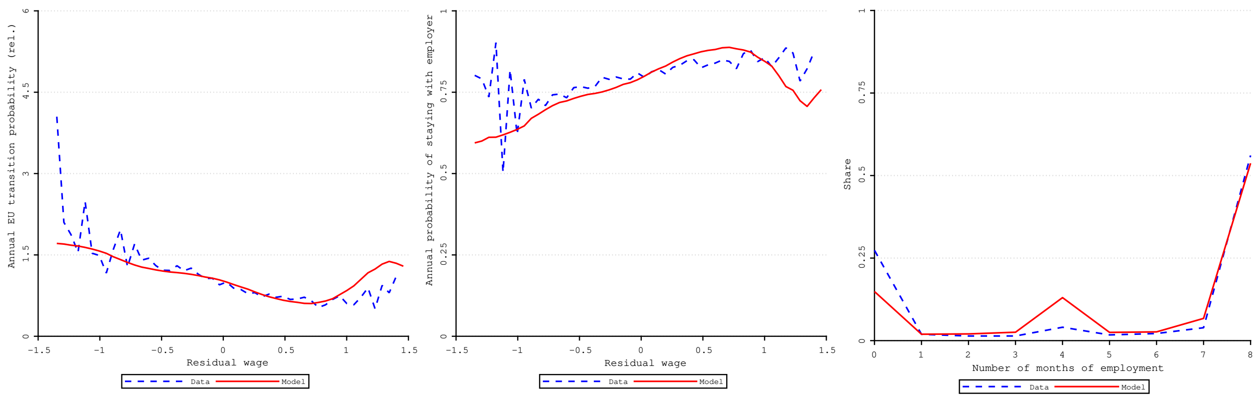


Figure 9 shows untargeted moments in the model and data. Panel (a) show the EU rate by wage relative to the EU rate at the median wage bin in the model (solid-red) and the data (dashed-blue). Panel (b) shows the share of workers who stayed with the same employer over the previous calendar year by wage in the ORG 4 in both the model (solid-red) and the data (dashed-blue). Panel (c) shows the distribution of workers over months of employment over the eight ORG months in the CPS in both the model (solid-red) and the data (dashed-blue).

Panel (c) presents the distribution of workers over months of employment during the eight-month CPS panel. Despite matching the high average monthly flow rates in the data, the model accurately captures the large share of respondents that are employed for all eight months of their survey. Permanent worker level heterogeneity in separation rates is key to this success. It understates the share of workers with zero months of employment, suggesting that incorporating heterogeneity in job-finding rates from non-employment could improve fit.[14]

4.4 Cross-state patterns

Table 1 presents the regression results from the third step of our estimation, which informs the number of firms per market, m. For completeness, we start in columns (1)–(2) with a version of regression (17) with the job-finding rate of the employed (λ^e) as the dependent variable. We find a negative correlation between the job-finding rate of employed workers and employer concentration, as measured by average firm size, after controlling for state and time effects. This suggests that as a state's labor market becomes more concentrated, workers receive fewer opportunities to move up the job ladder. This relationship remains stable over time—when we include a linear time trend interacted with firm size, the estimate is small and statistically insignificant.[15]

[14] We have explored such an extension but found that it had minimal impact on our main conclusions below.

[15] Across all our specifications, the time trend consistently lacks economic and statistical significance, and its inclusion has no meaningful effect on any of our other estimates. For this reason, we do not further report these results.

Additionally, columns (1)–(2) show that the job-finding rate of the employed is strongly positively correlated with that of the non-employed, with an estimated elasticity of 0.77. While this result aligns with theoretical predictions, there is no mechanical reason to expect this outcome. We hence interpret the strong within-state correlation between changes in λ and changes in λ^e as support for our methodology.

Column (3) presents a version of (17) with the job-finding rate of the non-employed as the dependent variable. Employer concentration does not exhibit a statistically significant correlation with λ. If these correlations reflect a causal relationship, this suggests that higher employer concentration has little impact on the job-finding prospects of non-employed workers but restricts job opportunities for the employed.

Columns (4)–(5) estimate (17) with the log difference between the job-finding rates of the employed and non-employed as the dependent variable. As we would expect based on the results in columns (1)–(3), this difference is strongly negatively correlated with employer concentration. The gap between these job-finding rates narrows when the job-finding rate of the non-employed is higher. One possible explanation is that increased job search efforts by employed workers crowd out opportunities for non-employed job seekers. Alternatively, measurement error in λ would introduce downward bias. Additionally, a higher separation rate is associated with lower relative search intensity among the employed, which is consistent with the idea that a higher separation rate discourages job search by reducing the expected duration of newly found jobs.

Column (6) estimates a version of (17) with the product of the separation rate and the job finding rate of workers on advanced notice on the left-hand side, $\ln \overline{\delta} + \ln \lambda^f$. It is not statistically significantly correlated with employer concentration. As we discussed above, an alternative interpretation of the separation shock plus notice period is that of job-to-job mobility in pursuit of other aspects than a higher wage. Under this alternative interpretation, we would conclude that higher employer concentration is associated with lower mobility toward higher paying jobs without a corresponding increase in mobility in other types of job-to-job mobility.

Finally, columns (7)–(8) correlate reduced-form outcomes as a validation of our methodology. Specifically, we project the overall job-to-job transition rate in the model on the fraction of employed workers who report working for a different employer than last month (conditional on being employed in both months), controlling for year and possibly state fixed effects

$$\ln jj_{sy} \quad = \quad \tau * \ln \text{switcher}_{sy} \; + \; \alpha_s \; + \; \alpha_y \; + \; \varepsilon_{sy} \tag{18}$$

This raw measure of job-to-job mobility is available since the redesign of the CPS in 1994.

Without state fixed effects—i.e. pooling demeaned repeated cross-sections—we estimate that a one percent increase in the raw job-to-job mobility rate is associated with a 0.48 percent increase in our measure of overall job-to-job mobility. Adding state fixed effects, the elasticity falls to 0.34. That is, the pass-through from the raw measure to our estimate is substantially below

Table 1: Cross-state regression results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\ln \lambda^e$		$\ln \lambda$	$\ln \lambda^e - \ln \lambda$		$\ln \overline{\delta} + \ln \lambda^f$	$\ln jj$	
β	0.026	0.026	-0.005	0.026	0.026	-0.653		
	(0.005)	(0.005)	(0.007)	(0.006)	(0.005)	(15.815)		
Trend		-0.000						
		(0.000)						
$\overline{\delta}$	-2.224	-2.226			-2.224			
	(0.059)	(0.059)			(0.059)			
λ	0.773	0.769			-0.227			
	(0.129)	(0.130)			(0.129)			
switcher							0.481	0.342
							(0.113)	(0.148)
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes	no	yes
Obs.	2,000	2,000	2,000	2,000	2,000	2,000	1,450	1,450
States	50	50	50	50	50	50	50	50
Years	40	40	40	40	40	40	29	29

Table 1 reports estimation results from the nonlinear least square regression (17) and the ordinary least squares regression (18). Standard errors are not adjusted for first-stage estimation error and not clustered by state.

one. However, the small number of observations at the state-year level likely implies substantial measurement error. Broadly, we view this evidence as supportive of our methodology.

4.5 Parameter estimates

Table 2 summarizes our parameter estimates by decade. We estimate that flows in and out of non-response have been relatively stable over this 40 year period. This may seem surprising given the well-known fact that survey response rates have been declining over time. The reason is two-fold. First, we drop respondents who never volunteer an answer from our analysis. Second, due to the issues discussed above of linking respondents over time in some years, we end up with a lot of missing values for employment status in a few years in the earlier part of the survey. In any case, since we assume that missing is random, the level of non-response is inconsequential.

We estimate a decline in the job-finding rate of the non-employed, λ, of approximately 15 percent between the 1980s and the 2010s. The share of employed workers on recall, ε, has remained stable over time. As we highlighted above, ε could alternatively be interpreted as measurement error in employment status, which suggest no pronounced change in such measurement error. However, we observe a significant increase in recall error, ν, in the March supplement, indicating that a larger share of workers today with a spell of non-employment in the past calendar year fail to correctly recall this.

Average wage growth with tenure, μ, does not follow a monotonic trend. We estimate an annual autocorrelation of wages of $e^{-12*\theta} \approx 0.85$, which has fallen modestly over time. At the same time, the standard deviation of wage innovations, σ, has increased.

Table 2: Parameter estimates

		(1) 1982–1991	(2) 1992–2001	(3) 2002–2011	(4) 2012–2021
Panel A. Pre-set					
α	elasticity of matches wrt vacancies	0.500	0.500	0.500	0.500
V	aggregate vacancies (Barnichon, 2010)	0.031	0.029	0.021	0.036
Panel B. Calibrated					
in	re-entry to being observed	0.123	0.111	0.116	0.142
out	rate of dropout from survey	0.156	0.144	0.123	0.167
λ	job finding rate, unemp	0.055	0.054	0.046	0.047
ε	share workers on temp. layoff	0.011	0.010	0.011	0.010
ν	recall error for stayer status (annual)	0.102	0.153	0.200	0.261
Panel C. Internally estimated					
μ	long-run mean wage	0.173	0.107	0.173	0.079
θ	autocorrelation of wage process	0.012	0.015	0.016	0.014
σ	s.d. of diffusion	0.193	0.220	0.233	0.236
λ^e	arrival rate of job offers	0.023	0.020	0.016	0.013
λ^f	job-to-job move upon separation	0.468	0.558	0.548	0.554
ω	difference in offered wage btw types	0.104	0.132	0.011	0.132
δ^1	separation rate, low type	0.088	0.093	0.095	0.091
δ^2	separation rate, high type	0.010	0.007	0.007	0.011
Panel D. Cross-state					
β	rel. between # markets & # workers	0.026	0.026	0.026	0.026
Panel E. Implied					
$\overline{\delta}$	overall separation rate	0.030	0.026	0.027	0.031
δ^s	het. in sep. rate, $\delta^1 = \overline{\delta}\delta^s$; $\delta^2 = \overline{\delta}/\delta^s$	2.943	3.528	3.567	2.883
ϕ	rel. search intensity, $\lambda^e = \lambda\phi(m-1)/m$	0.872	0.814	0.808	0.677
ξ	length of notice period	11.432	15.150	17.306	17.290
χ	matching efficiency, $\lambda = \chi(V/S)^\alpha$	0.301	0.294	0.291	0.215
V/S	labor market tightness	0.034	0.034	0.025	0.047
m	recruiting employers per market	1.938	1.830	1.830	1.780

Table 2 reports the estimated model parameters by decade expressed at a monthly frequency unless otherwise noted.

We estimate that roughly 2.3 percent per month of employed workers in the 1980s receive a voluntary outside offer, but that this has fallen to only 1.3 percent in the 2010s. We would expect a decline given the fall in λ discussed above, but the decline in λ^e is larger. Consequently, it must be that either search intensity of the employed ϕ or the number of recruiting employers m also has declined. In fact we find significant declines in both, with search intensity of the employed having declined by 22 percent and the number of recruiting employers by eight percent. Our estimates imply that employed workers search with 68–87 percent of the intensity of non-employed workers, while a market on average has 1.85 recruiting employers over our sample period. As a point of reference, Azar et al. (2020) estimate based on online vacancy postings that the average labor market—defined by occupation and commuting zone—has 2.3 recruiting employers.

About 50 percent of workers on advance notice find an alternative job before their separation is realized. This share has risen over time. Combined with our relatively low estimated job finding rate λ, this implies that workers know roughly a year in advance that their job will terminate. Given a pretty stable separation rate, this implies that a larger share of workers today make job-to-job transitions toward jobs that do not necessarily pay better. As we discussed above, this could alternatively be interpreted as more workers moving in pursuit of other aspects than the wage.

Figure 10 plots the wage and wage offer distributions in the model. As highlighted by panel (a), the true offer distribution recovered based on (16) is shifted to the left of the distribution of wages among those who were non-employed in the previous month. The reason is that some employed workers are recalled to their previous employer after a brief spell of non-employment, leading them to be recorded as hires from non-employment. Since we assume that recall is independent of worker type and the wage, recalled workers have a wage distributed according to the overall wage distribution. Consequently, their inclusion in the pool of hires from non-employment shrinks the gap between the observed wage and wage offer distributions, so that the true offer distribution must be further to the left.

High-type workers receive better wage offers than low-type workers. Furthermore, they are less likely to experience a separation shock, leading to an even larger gap in earned wages (panel (b)). Since high-type workers sample better wage offers and are overrepresented in the pool of employed, employed workers on average sample higher wages than non-employed workers, as illustrated by panel (c). This is consistent with the evidence in Faberman et al. (2022).

Figure 10: Wage offer and wage distributions

(a) Offer distribution (b) Wage and offer distributions (c) Offer distributions

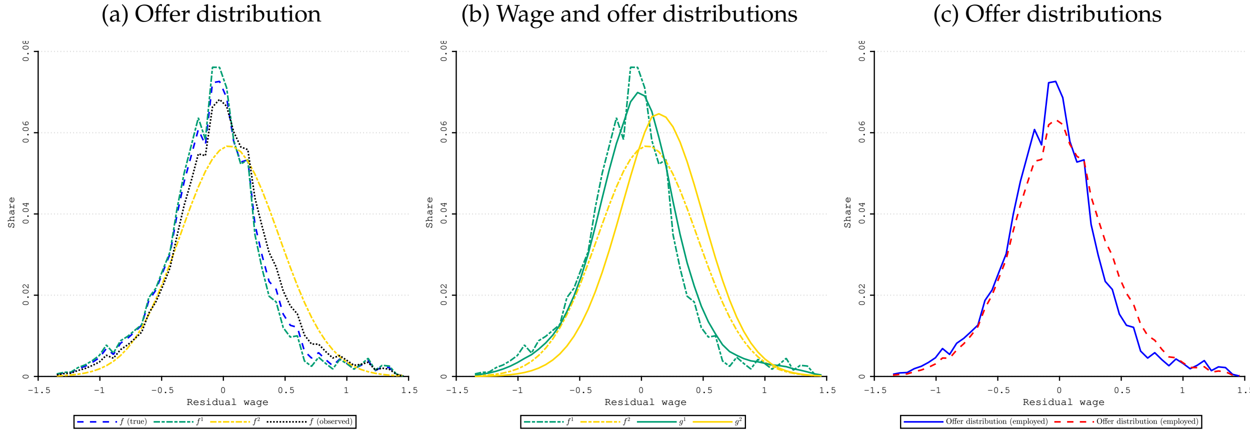


Figure 10 shows the wage and wage offer distributions. Panel (a) plots the overall offer distribution and by worker type, as well as the distribution of wages among workers who were recorded as non-employed in the previous month. Panel (b) plots the wage and wage offer distributions by worker type. Panel (c) plots the offer distribution of the non-employed and employed in the model.

Panel (a) of Figure 11 presents our estimates of the job-finding rate of the non-employed (λ), the job-finding rate of the employed (λ^e), and reallocation shocks ($\delta\lambda^f$) based on decade-long subperiods, as well as an alternative estimation using annual data. The annual data are smoothed using a nine-year centered moving average to highlight long-term trends. Several key observations emerge for the estimated job-finding rates. First, as we already noted above the job-finding rate of the employed declined significantly over time. The annual data, however, suggest a reversal after the Great Recession. Second, while there is no mechanical reason to expect this, the job-finding rate of the employed closely tracks that of the non-employed, aligning with the theoretical prediction that the two should be linked through $\lambda^e = \phi\lambda(m-1)/m$.

Figure 11: Job offer arrival rates and realized flows in the model

(a) Job finding rates (b) Realized EE mobility

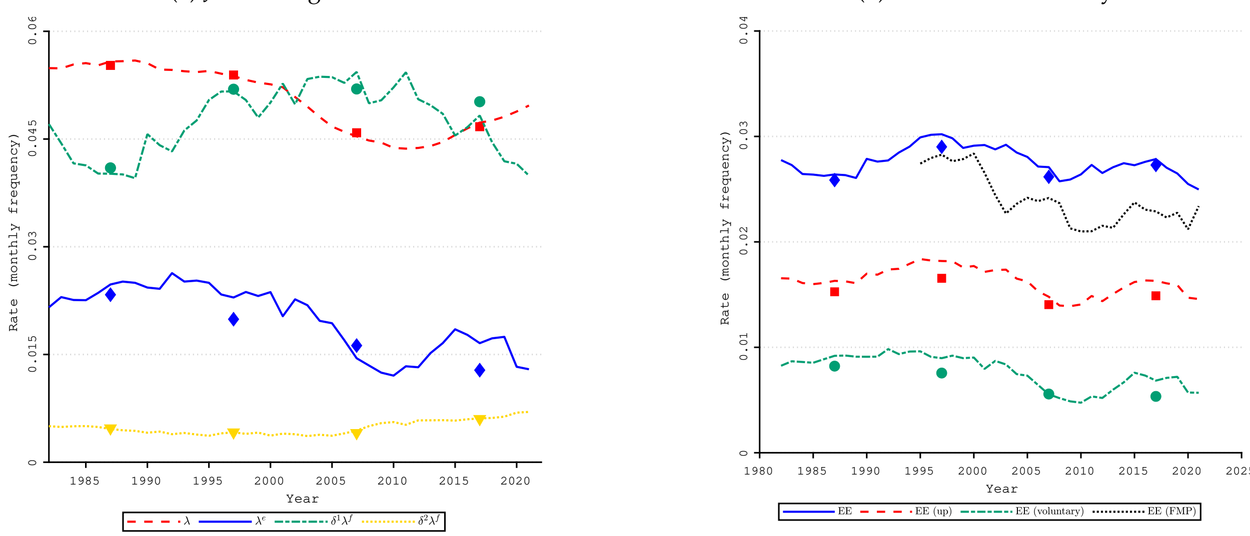


Figure 11 shows the evolution of key estimated labor market moments in the model. Markers represent estimates from our decadal estimation, while lines represent smoothed results from our annual estimation. Panel (a) presents job-finding rates for unemployed (dashed-red), employed (solid-blue), and workers on separation notice (dash-dotted-green), with 9-year moving averages and decade-pooled estimates (markers). Panel (b) displays the realized EE mobility rate for all workers (solid-blue), those moving to higher wages (dashed-red), voluntary job switchers (dash-dotted-green), and the data equivalent using the adjustment in Fujita, Moscarini and Postel-Vinay (2024) (dotted-black).

Panel (b) illustrates the implications of these trends for realized mobility, revealing several interesting patterns. First, our overall measure of job-to-job mobility matches well the raw data series constructed by Fujita, Moscarini and Postel-Vinay (2024), both in terms of levels and time trends, although our series shows a somewhat less pronounced decline.

Second, unsurprisingly given the decline in the voluntary job-finding rate of the employed, its associated mobility rate fell. However, the decline was less pronounced than the reduction in the arrival rate of offers, decreasing by 35 percent compared to a 45 percent decline in the offer arrival rate. The reason is that as the arrival rate declines, workers become increasingly mismatched, making them more likely to accept outside offers.

Third, job-to-job mobility directed toward higher-paying jobs accounts for only about a third of overall job-to-job mobility. A corollary is that it is difficult to learn much about trends in job-to-job mobility that systematically relocate workers to higher-paying jobs based on overall job-to-job mobility.

Fourth, since separation shocks followed by an immediate job-to-job transition sometimes move workers into higher-paying positions, the realized rate of mobility toward higher-paying jobs exceeds the voluntary job-to-job mobility rate. On average, we estimate that 59 percent of job-to-job transitions resulted in a wage gain in the 1980s, declining to 55 percent in the 2010s. These estimates broadly align with SIPP data, where it is possible to observe associated wage changes.

4.6 Counterfactual analysis

We use the estimated model to understand and quantify the impact of various forces on average wages over the past four decades via a series of counterfactual exercises.

The gap between the wage and wage offer distributions. We start by analyzing the forces behind the gap between the wage and wage offer distributions. To that end, we resolve the model setting one particular parameter to zero (one) while holding all other parameters fixed at their estimated values, and compute by how much the gap shrinks. We label this as the contribution of those particular forces.

Table 3 decomposes the gap between wage and wage offer distributions into the contribution of various forces. For instance, to quantify the role of unobserved heterogeneity, we set $\omega = 0$ and $\delta^s = 1$, effectively eliminating differences in wage offer distributions and separation rates across workers. Keeping all other parameters fixed, we find that this shrinks the average gap by on average 35 percent, with some variation across decades.

To quantify the role of on-the-job wage growth, we perform analogous calculations by setting $\mu = 0$ while holding all other parameters fixed. This shrinks the wage gap by 25 percent on average, again with some variation across decades. Finally, to quantify the role of job to job mobility, we set $\phi = 0$ holding all other parameters fixed. This reduces the gap by on average 43 percent, again with some variation across decades.

We conclude based on this exercise that job-to-job mobility is the most significant driver of the gap between the wage and wage offer distributions, with unobserved heterogeneity a close second. Wage growth on the job, while still relevant, plays a comparatively smaller role. If we shut down all three of these margins jointly, the gap almost completely closes, indicating that these three forces account for the vast majority of the gap between the wage and wage offer distributions (the remaining portion is attributable to factors such as wage volatility, σ).

Table 3: Decomposition of the Average Gap Between Offer and Wage Distributions

	1982–1991	1992–2001	2002–2011	2012–2021
Gap in the data (log points)	0.144	0.122	0.114	0.096
Gap in the model (log points)	0.134	0.119	0.108	0.089
Decomposition				
Unobserved heterogeneity (ω, δ^s)	32.5%	38.3%	28.0%	41.4%
On-the-job growth (μ)	25.3%	20.1%	39.4%	18.9%
Job-to-job mobility (ϕ)	43.9%	46.6%	34.1%	45.9%
All three combined	90.2%	88.3%	87.2%	89.2%

Table 3 reports contributions of different channels to the average gap between wage offer and overall wage distributions for each of the past four decades. The channels considered are (i) unobserved heterogeneity, (ii) on-the-job wage growth, (iii) job-to-job mobility, and (iv) all three combined.

Changes in real wage growth. We next use the model to understand aggregate wage trends over the past decades. Recall that the data and model used above is denominated in the average residual wage of hires. To recover the level of wages over time, we project real wages of hires on race, gender, age, education, state, three-digit occupation, month of survey and year fixed effects

$$\ln wage_{it} = \alpha_r + \alpha_g + \alpha_a + \alpha_e + \alpha_s + \alpha_o + \alpha_m + \varepsilon_{it} \tag{19}$$

Panel (a) of Figure 12 plots the average composition-adjusted real wage of hires based on (19) as well as average composition-adjusted real wages of all workers over time in the model and data. We construct the latter by adding to the average composition-adjusted real wage of hires the gap between the wage and wage offer distributions constructed above.

Our counterfactual exercises involve fixing some parameters at their estimated values in the 1980s, and letting all other parameters evolve according to their estimated time-varying values. We compute the impact of these changes in real wage growth by adding average wages of hires from non-employment. That is, we assume that real wage growth of hires from non-employment remains unaffected by the changes in parameters that we consider. We view this assumption as conservative, for the following reason. If, for instance, we had been able to freeze the level of employer concentration at the lower level observed in the 1980s, we find it plausible that wages of hires from non-employment, if anything, would be higher in the 2010s relative to the data. Similarly, if we had been able to freeze aggregate matching efficiency at its higher level in the 1980s, we find it plausible that real wage growth of hires would have been greater. Since we do not incorporate such effects, we obtain a lower bound.

Table 4 summarizes our results.[16] Changes in on-the-job wage dynamics contributed significantly to weak wage growth between the 1990s and the 1980s, primarily driven by a decline in on-the-job wage growth (μ). While on-the-job wage growth rebounded in the 2000s relative to the

[16] In the interest of space, we present only the key drivers of wage dynamics over this period. Because the type-specific separation rates did not change all that much over this period, holding these parameters fixed at their 1980s values would do little to wages in 2010s.

1990s, it declined again in the 2010s relative to the 2000s. Cumulatively, we estimate that if on-the-job wage dynamics had remained as they were in the 1980s (while other parameters evolved as in the data), real wage growth between 1982–1991 and 2012–2021 would have been 1.9 percentage points higher.

If the parameters governing job-to-job mobility had been held fixed at their 1980s values, real wage growth would have been 3.3 percentage points higher. Of this, a change in the probability of finding a new job during the notice period, λ^f, contributed to a 0.6 percentage point fall in real wage growth.[17] It contributed to a fall in real wage growth, since we estimate the workers are more likely today to make an involuntary transition toward lower paying jobs.

Figure 12: Wage growth under counterfactual scenarios

(a) Wage growth in data and model

(b) Counterfactual wage growth

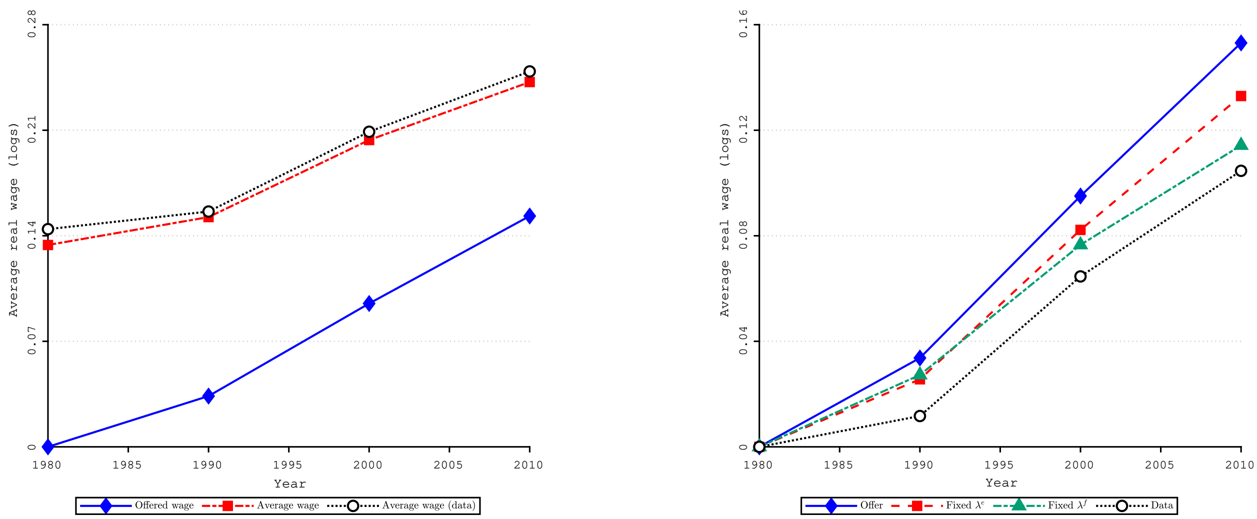


Figure 12 shows the composition-adjusted growth in average real wages of hires from non-employment and all workers in the model and data (panel (a). Panel (b) shows the counterfactual growth in average real wages holding some parameters fixed at their values in 1982–1991, while letting all other parameters evolve as estimated.

Decomposing this further, if matching efficiency had been held fixed at its higher level in the 1980s, real wage growth would have been 1.1 percentage point *lower* through the channel of involuntary job-to-job mobility. The reason is that the associated higher λ would have led to more involuntary job-to-job transitions. Since such transitions contribute to lower wages, real wage growth have been lower under this counterfactual. If the notice period had been held fixed at its shorter length in the 1980s, there would have been fewer job-to-job transitions toward lower paying jobs, so that real wage growth would have been one percentage point higher.

A change in the effective job finding rate of the employed, $\lambda^e = \lambda\phi(m-1)/m$, contributed a 2.5 percentage point decline in real wage growth. However, we caution that while wage growth would have been higher if we had held fixed involuntary mobility at its lower value in the 1980s, it

[17]We show only the effect of a change in λ^f and not $\delta^i\lambda^f$. Because we estimate only small changes in the type-specific separation rate δ^i, it has only a small effect on wage dynamics over this period.

is possible that the increase in such mobility improved worker welfare in some other dimension. Panel (b) illustrates. Decomposing this further, the fall in matching efficiency contributed a 1.3 percentage point fall in real wage growth. In contrast, because labor market tightness first fell but subsequently rose, cumulatively it contributed to real wage growth over the 40 year period.

If search efficiency or intensity of employed workers had been held fixed at its value in the 1980s, real wage growth would have been 0.9 percentage points higher. There are several interpretations of this finding. On the one hand, it might reflect a deteriorating search technology for employed workers, in which case the associated decline in wages might reasonably capture its welfare implications. On the other hand, it might be the result of less costly search effort of employed workers, in which case the associated wage change might overstate its welfare consequences, since it does not factor in the benefit of less search effort. Given that the objective of this paper is to understand the contributions of a changing labor market structure toward average wages, we note this caveat but do not attempt to further quantify its welfare implications. If employer concentration had been held fixed at its lower value in the 1980s, real wage growth would have been 0.6 percentage points higher.

Table 4: Cumulative effect of changes in labor market parameters on wages (percent changes)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
					Job-to-job									
	On-the-job				Involuntary				Involuntary					
	μ	θ	σ	all	χ	V/S	ξ	λ^f	χ	V/S	ϕ	m	λ^e	all
1990s	1.5	0.4	-0.1	**1.6**	-0.1	-0.0	0.9	0.9	0.1	0.0	0.3	0.3	**0.7**	**1.6**
2000s	0.0	0.0	-0.2	**-0.2**	-0.1	-0.6	1.2	0.7	0.1	0.5	0.2	0.3	**1.3**	**2.0**
2010s	2.0	0.2	-0.2	**1.9**	-1.1	0.4	1.0	0.6	1.3	-0.5	0.9	0.6	**2.5**	**3.3**

Table 4 summarizes the counterfactual growth in average real wages holding some parameters fixed at their values in 1982–1991, while letting all other parameters evolve as estimated.

5 Conclusion

We quantify the impact of changes in the structure of the U.S. labor market on real wage growth over the past 40 years. Based on a rich model of worker dynamics and publicly available data from the CPS, we estimate that a decline in the net mobility rate toward higher paying jobs reduced real wage growth by 3.3 percentage points between the 1980s and the 2010s. This is in turn the result of greater job-to-job mobility toward lower paying jobs (0.6 percentage point reduction in real wage growth), lower matching efficiency (1.3 percentage point fall in real wage growth), less efficient or intense search by employed workers (0.9 percentage point fall in real wage growth), and greater employer concentration (0.6 percentage point fall in real wage growth).

Our work suggests at least two directions for future work. First, our analysis treats average wage growth of hires from non-employment—or more generally the wage offer distribution—as unaffected by the structural changes to the labor market that we consider. We find it plausible that the decline in matching efficiency or the increase in employer concentration might also depress wages of hires from non-employment, in which case our estimates understate the true effect of greater employer concentration on wages. It would be interesting to relax this assumption.

Second, we estimate that less efficient or intense search of the employed and more frequent job-to-job mobility toward lowering paying jobs have reduced average wages. From a welfare perspective, however, they might also come with the benefit of less effort spent on job search and improvements in non-wage aspects of jobs. It would be useful to further assess the welfare consequences of these changes.

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